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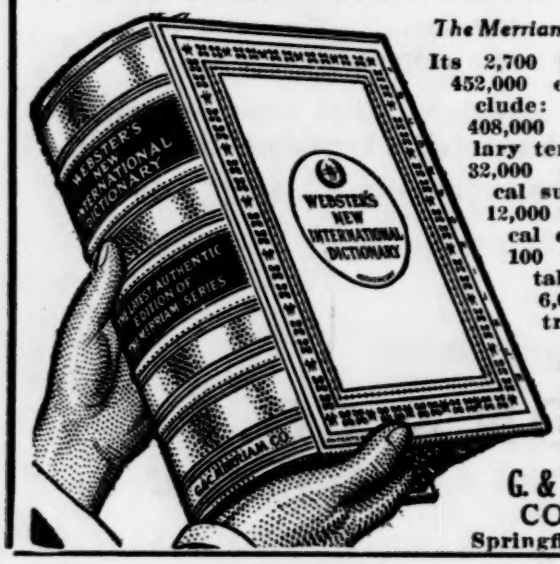
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SPECIALIZATION AND COOPERATION IN SCIENTIFIC RESEARCH[1]

THE three great functions of a university are to train young people in the art of living, to guide them in the search for truth and actually to engage in the pursuit of truth. The first two of these are universally agreed upon and endorsed, but not so universal and whole-hearted is the recognition of the third—the research function of an institution of higher learning. In some quarters the research aspirations of a university meet with distinct disapproval as encroaching upon the supposedly more serious business of the institution. In other quarters they are viewed with grudging or amused tolerance as harmless little idiosyncrasies in which scholarly men must be indulged in order to keep them contented and out of mischief. In the more enlightened quarters, however, it is realized that a university can not best perform any of its functions or measure up to its opportunities unless full and ungrudging support is given to its attempts to advance human knowledge.

In such a gathering as this I need make no defense of research as a valuable and legitimate activity of a university. It may not be out of place, however, briefly to review some of the considerations which are involved. These naturally fall under three heads, of which the first is

I. *Advantage to the student.* If ability to think clearly and independently, to organize all his available mental resources, to direct his best efforts for the purpose of solving problems and meeting difficult situations is a thing worth gaining, then research is an important part of a student's training. For the carrying on of research and its success depend on the degree to which these qualities of the mind can be focused on the problem. And what qualities of the mind are more important to develop than these? They can not be developed by that woefully overdone method of teaching by the easier process of fact-cramming. To learn a thing one must do it. To develop these mental traits they must be exercised. And the exercise of these mental traits, focused on some problem, *is* research.

Another advantage of research to the student is to be found in its stimulation of his interest. Nowadays people are not interested in things static, they

[1] Address delivered at the Founder's Day Exercises, Lehigh University, on October 5, by Professor K. T. Compton, Princeton University.

must be moving ahead. Our age is looking forward, not backward. For this reason we teachers find our students lukewarm or rebellious if confronted with the prospect of a long, long study of facts, laws and events for their own sake, without any immediate objective. But if we can put the student on his mettle by arousing his curiosity or interest in something into which he feels that he can put his creative and critical personality, it is rare indeed that he fails to respond.

I was at one time on the faculty of a small new college in the far west. Here not only was every graduating senior supposed to present a thesis embodying some original research in the field of his major subject of study, but class study itself was largely built around the idea of original investigation. I have never been in such an atmosphere of undergraduate intellectual activity. Real study was not neglected but stimulated. The objects of this undergraduate research were, of course, of various values. But whether it was sleeping, eating and talking with the army of the unemployed in the old Billy Sunday tabernacle in order to investigate the causes of their economic troubles or whether it was attempting to predict the weather from observations of wind, barometer and length of a horse hair, it *did* lead to eager study and independent judgment, and it led a very large proportion of the students into post-graduate study.

A few days ago I was talking with a chemist who told me how he happened to take up the study of sugars and other carbohydrates. During his senior year he decided to go on with post-graduate study, but did not know exactly what he wanted to do. He was just generally interested in science. His professor of physics had asked him to assist in some research work on the solubility of a certain kind of sugar by testing its purity by the well-known method of measuring its power to rotate the plane of polarization of light. In doing so he noticed that he obtained a variable result depending on whether he measured this effect immediately after dissolving the sugar or waited some time before making the measurements. The professor then suggested that the further investigation of this phenomenon might be a good thing to follow up. His professor of chemistry, on the other hand, pooh-poohed such a project as trivial and almost certain to be principally a waste of time. He urged this student to take a good advanced text-book of organic chemistry and, starting at its beginning, go right through it and actually make a sample of every organic substance whose method of preparation was described. In this way, correctly argued the professor, he would become a skillful and learned chemist.

Now the investigation proposed by the physics professor *might* have been of trivial importance, but the idea of doing something new appealed so strongly to the student that he chose to try it. The result of this work was a fundamental discovery regarding the nature of sugar which has opened up a life's work for this man, made him one of the three world's authorities in this field, and shows promise of throwing light on some of the obscure phenomena of digestion and metabolism.

The second consideration to which I would call attention is

II. *Advantage to the teacher.* Of course anything which benefits the student is at the same time an advantage to the teacher. The preceding remarks may therefore be considered as applying also under this heading. In addition I would call attention to the pedagogical value of research in maintaining the enthusiasm, confidence and prestige of the teacher. Concerning his enthusiasm and confidence nothing need be said, for it is obvious that a man possesses these qualities especially in regard to a subject to which he himself is contributing. If you wish to learn something about houses, talk to a man who is building one; if you wish to learn something about textiles, talk to a textile manufacturer, or his shop foreman, not to a clerk who is paid to sell textiles over the counter and who knows just enough to answer the questions usually asked by customers; if you wish to know some field of knowledge, go to a man who is contributing to it.

As regards the prestige of a teacher, his general standing in the community and the regard in which he is held by his students much might be said, but I shall make only one suggestion. It is well known that the general prestige of a university professor in Europe is far above that in this country. Der Herr Professor Geheimrat is a power in the country and an ideal to his students, most of whom would give anything to become such as he. How many of our students envy us our positions? Not so many, I fear. To what is the difference due? I would suggest two elements of the situation which appear to me to deserve consideration.

In the tremendously rapid development of our country we have always had room for expansion and had at our disposal vast untouched natural resources from which to draw. Consequently great industries have been based upon exploitation of these resources and the whole tenor of our business life is permeated with the idea of success to the pusher—to the aggressive, quick-witted and perhaps not too scrupulous fellow who knows a good thing when he sees it. The American idol has been success, meaning usually business success. There are thousands who have

attained this success by our so-called keen business methods to one who has attained it through scholarly intellectual effort—and our country is still young and undeveloped enough to stand it.

In the older countries, however, equilibrium has been reached. The natural resources are limited and their conservation is a momentous question. Competition is keener. It is universally recognized that the economic outlook is dependent on the wisest (not the fastest) utilization of resources. Hence the whole industrial structure is far more dependent on research than is the case in this country, and the value of research is correspondingly more universally recognized. So the university professor, who there is preeminently a research man and trainer of research men, is recognized as an invaluable asset to the community.

I believe that a second element in the situation is the very rapid growth of the schools in this country. It has been impossible to train teachers or increase budgets rapidly enough to keep pace with the demand. Consequently the average fitness of both our teachers and our methods leaves much to be desired. I do not wish to decry our school system, for it has developed magnificently considering the magnitude of the difficulties to be overcome. But I believe that the consensus of opinion among those who have studied the situation (as opposed to the popular conception) is undoubtedly that, as regards *quality*, the education in a number of the European countries is distinctly superior to ours. And if we fail to demand and bring out the best in our students, we fail to the same degree to earn their respect and that of the community.

Now, in the field of higher education, the fundamental difference between our usual methods of education and those in vogue in the European countries to which I have referred is found in the degree of independence and responsibility expected of the student. Here we have too largely carried over the secondary school methods of mass production, involving frequent tests, marks and examinations, careful assignments of blocked out lessons, adherence to textbooks, supervision of attendance and study. There the student is set to master a subject. How or when he does it, whether through the aid of the professor's lectures or not, is not of much consequence. The big requirement is that he pass a comprehensive examination at the end. He knows that if he fails it is his own fault, and also that the choice positions in professional and government service will go to those who pass with highest distinction. He has therefore the incentive to exert every effort, and he must do it to a large extent independently. This independence of study, which culminates in research, inevitably leads to respect for the men who have contributed to the subject which he studies. The professor is thus a guide and example rather than a taskmaster or tutor.

As evidence of the truth of these remarks and as a hopeful sign for the future I may call attention to the increasing tendency in the colleges of this country to increase both the excellence of achievement expected of the student and the amount of freedom and responsibility accorded him in his endeavor to attain it. This movement is usually undertaken somewhat timorously and in small steps, but I have yet to hear of a place where it has been tried and has not met with marked success and approval both by students and faculty. I am therefore firmly convinced that, in the field of higher education, we have greatly overestimated the importance of pedagogical systems and have greatly underestimated the possibilities inherent in a mutual relation of student and professor as common searchers for the truth.

In this brief analysis of the importance of research, my third heading is

III. *Advantage to the community.* Inventors in this country have always been popular idols. We tell young school children about the inventions of Robert Fulton, Eli Whitney and Thomas Edison. We have been blessed by a number of men who had the spark of genius to conceive of a steamboat, a cotton gin, a dynamo or an incandescent lamp and numerous other machines and processes on which so much of life to-day depends. Nothing in the world is so potent with possibilities as a new idea, and really new ideas are rare and the product of genius. (Not all inventions are of this class. Though I occasionally enjoy Life Saver candies, I do not believe that the invention of the hole in the candy deserved the reputed reward of a million dollars, or that the invention of a blue stripe on kitchen utensils should establish a man in either Wall Street or the Hall of Fame.)

As I have said, we have always lauded the inventor. But both behind and in front of the inventor is the true research worker. The research worker first makes a fundamental discovery; then he proceeds to investigate it in all its aspects and attempts to explain it in its relations with other known phenomena. Next the inventor sees some way of turning this discovery to some practical account—and *this* is the step ordinarily called invention. Finally other research men investigate ways in which this practical application of the discovery may be made most efficient and effective. Who, in this chain of activities, deserves the credit? The patent and the right to financial rewards go to the inventor, and I should be the last to try to belittle the value of his work. My plea, however, is for greater realization of the fundamental

and indispensable character of real scientific research, both as prerequisite to inventions and as essential to their perfection.

Permit me to give one or two illustrations, which I choose from my own field of science, though I realize that equally striking illustrations could be drawn from other fields by those familiar with them.

Back in the seventeenth century a number of French, English and Italian scientists investigated a newly-discovered phenomenon, which was that air in the neighborhood of hot objects was not a good insulator but could conduct feeble electric currents. The phenomenon appears to have attracted no further interest until Guthrie, in 1853, showed that a red hot iron ball, suspended in air, could retain a charge of positive electricity but not one of negative electricity. About thirty years later two Germans, Elster and Geitel, again took up a study of this phenomenon and with characteristic German thoroughness, investigated the electric currents obtained in all kinds of gases in the neighborhood of a variety of heated metal wires. Then Edison found similar currents of electricity flowing from the filaments of his newly invented carbon filament lamps into the surrounding partially vacuous space. These and numerous similar investigations led to no satisfactory explanation of the phenomenon.

But, about this time, the discovery of X-rays by Roentgen, of radioactivity by Becquerel and the researches of Sir Wm. Crookes led to the discovery of electrons and largely at the hands of Sir J. J. Thomson and his pupils there was developed a coherent theory capable of explaining the known facts of electric discharges in gases and of predicting many new phenomena hitherto unsuspected. Among other things, Thomson showed that the electrical conductivity of gases in the neighorhood of hot wires is due to electrons which are emitted from the wires, and that this emission occurs even in the best vacuum. Then one of Thomson's most brilliant pupils, O. W. Richardson, developed a theory to explain this emission of electrons and, in a remarkable series of investigations carried on partly in England and partly at Princeton University, he arrived at a very good understanding of the various factors which control this emission of electricity from hot bodies. As far as I have been able to verify the matter, every investigator who had made an important contribution to the discovery and study of the phenomenon up to this point had been a university professor or his pupil, with the single exception of Edison.

Now the story of how this was put to practical use has been the subject of much patent litigation and there were doubtless a number of nearly simultaneous steps in the matter. As I heard the story it is as follows, which, if not exactly true in detail, is certainly true in principle. Richardson delivered a lecture before an Electrical Engineering Society. At the close of the lecture an electrical engineer came up to him with a pencil sketch on the back of an envelope and asked whether the filament and metal electrodes in a glass bulb, there shown, would not function as an electric valve, to permit electric current to flow in only one direction. This suggestion was an invention, the forerunner of all our radio tubes, certain types of battery chargers, important links in long distance telephony, special furnaces for careful metallurgical processes, and modern X-ray apparatus. Now there are literally thousands of men engaged in research on this subject, which has drawn on the engineer, physicist, mathematician, chemist, geologist, biologist and physician. The importance to our civilization of the modern means of communication made possible by this work can scarcely be conceived.

I have gone into this example in some detail. Some other illustrations I have time only to suggest. Their development has gone through the same general stages as in the case just discussed.

The entire industry of electrochemistry is built upon the discoveries of Faraday, a professor at the Royal Institution.

Joseph Henry, first a teacher in a boys' school, then professor of physics at Princeton, constructed the first real electromagnet, the first telegraph and printing telegraph, had a wireless set with which his family used to call him from the laboratory to his meals and, most important of all, discovered, jointly with Faraday, the laws of electromagnetic induction which underlie all electric power machinery. And when urged by his friends to press his claims for patent rights he answered that his scientific work was too important to be hampered by attending to such trivial matters.

In the electric light industry, the single discovery of a method to make tungsten wire, which involved some years of painstaking and disheartening research, is conservatively estimated to save the American public $1,000,000,000 per year in electric light bills.

Of course not all our industrial life is dependent to the same extent on research. At the head of the list I would place such industries as the Bell Telephone Company, the radio companies and great chemical firms such as Du Ponts. At the bottom of the list would come those based on exploitation of natural resources, such as coal, steel, lumber and farming. Yet in all these the time is rapidly approaching when research will become of predominating importance in enabling the industries to meet the demands made upon them. An interesting example of

this is found in the oil industry. Just after the war two young university instructors conceived the idea of utilizing certain well-known laboratory instruments and methods for the location of oil fields. They made some tests to prove its feasibility and talked about taking out a patent, but, being inexperienced, decided first to consult a prominent oil geologist. He expressed interest and approval of their method, but discouraged them by saying that oil geologists now know all that this method could give in regard to oil location, so that such a device would be quite superfluous. So they dropped the matter. Now, less than ten years later, this very method is in use together with several other applications of laboratory devices. The oil companies are obtaining an astonishing record of "strikes" by their use and are vieing with each other to secure properly trained men and equipment to beat their competitors in the survey of new fields.

If we turn to biological and medical science, we come to a field which is almost free from industrial exploitation, but which is even more fundamentally important than are the physical sciences to human welfare. In these fields the only possible advance comes through research, and research is done almost exclusively in such altruistic institutions as universities, hospitals and foundations such as the Rockefeller Institute.

These illustrations bring out with striking emphasis three facts: (1) Inventions were preceded and followed by a very great amount of research work. (2) The research work which paved the way for the inventions was carried out almost exclusively in universities. (3) The entire body of work has resulted in the establishment of large industries and in inestimable opportunity and profit to humanity.

Let me in another way emphasize our debt to research. Sociologists have pointed out the important part played by slavery in the development of civilization. It was only when the ablest men's routine work was done by slaves, giving them leisure time to think, that those ideas were evolved which resulted in the physical and spiritual uplift of the race. To-day it is estimated that our practical applications of scientific discoveries have a producing power equal to sixty slaves for every man, woman and child. Compare, then, our present life with that of our primitive ancestors, and you have a vision of our direct and indirect debt to research.

Let us turn now to some practical aspects of the situation which have a bearing on our attitude toward research in the future. I wish very briefly to call attention to four conclusions which seem to be certainly justified by past experience. The first of these is that:

I. *Research must be more generally encouraged and supported.* You may have thought, in my illustrations of the advantages which have been derived from research, that I gave undue emphasis to invention and commercial development. I did so purposely, for I wished to bring out a contrast. Where immediate financial returns are in sight, the keen search for profits which spurs our business life brings quick support and reward. Thus industrial research and development are coming more and more to be looked upon as shrewd business policy. Purely scientific research, which is absolutely prerequisite and basic to invention and development, is, on the other hand, generally carried on at a personal sacrifice and cramped for facilities. The average scientist has to battle against the odds of other supposedly prior duties in order to get time for concentrated thought and sustained experimental effort. How short-sighted is this policy, which starves the roots of our future progress! Yet it is easily understood. The results of purely scientific inquiry are uncertain. There are many trivial steps for one great stride in advance, and there is no foretelling in which direction this stride will be. No ordinary business organization which has its vision fixed on the profits of the next few years can, as a selfish business proposition, support pure and independent scientific inquiry, because the chances of a return within this time and in the range of its interests are too remote. A few only of the big industries do support pure research from altruistic motives because they are able and willing to return to pure science a little in return for the benefits derived from it; or they may support it because their interests are so varied that they can justify taking a chance on some discovery that can be turned to profit; or they may support it for reasons of publicity and personnel. Whence, then, is support for scientific research to be obtained?

In the first place this support should come from those same funds which support the other activities of our universities, on the ground that research is an essential element and method of education. In the second place it will come from altruistic citizens and organizations who possess the wisdom and imagination to visualize the possibilities which research will uncover for the future. In the third place, as I have suggested, it should come from industrial and business organizations. I should like to see all such enterprises which profit directly from the work of science taxed to support research. Probably this can never be done by legal means, but its equivalent may be brought about by publicity and the force of enlightened public opinion. If, for example, a large proportion of such industries could be induced to share the burden of supporting research and this were done on a large scale, no one industry would be handicapped in re-

spect to the others and from the widespread nature of the research thus made possible, every industry would have a reasonable expectation of obtaining something to its direct advantage in addition to the indirect benefit of general business stimulation. Such is the justification of the "Hoover Fund" which the National Academy of Sciences is attempting to raise from large industries with the strong backing of Secretary Hoover. Finally, support may come from the government, if this can rise above the pressure of political struggle and popular appeal for tax reduction, and look to the welfare of the future. True, the government is now supporting a few research agencies, but on a very niggardly scale, in very restricted fields, and with such pressure for immediate practical returns as to drive out those scientists who might do big things and almost to kill the possibility of that type of research which might have great consequences.

It has been said that civilization is measured by the degree to which the people will sacrifice present desires for the sake of future benefits. On this basis the degree to which we support research should count heavily in estimating our degree of civilization.

The second conclusion which I would draw is that:

II. *The universities must be the chief agents and mainsprings of research.* There is no organization other than the universities where there are gathered together the men of scholarly training capable of carrying on research in the whole field of scientific inquiry. If there *were* some other suitable organization, it is there, rather than to the universities, that young men would go for research training, and this organization would at once *become* a university. Such work is undoubtedly the function of the universities.

Furthermore, it is a well-known paradox that the discoveries which have led to the greatest practical results have not been made by the men who were seeking the practical results. The reason for this is plain. The man who is seeking a certain end is mentally circumscribed by the methods which are already known for attaining this end. He may perfect some previous process, or he may see how to apply some phenomenon or principle which has not hitherto been applied, but his mental state handicaps him in discovering anything fundamentally new. The basic research must, therefore, be a free and unfettered search for truth. It is the universities alone which can offer any considerable opportunity for such endeavor.

Besides this, it is to the universities that the industrial laboratories and government bureaus must turn for their trained men. The head of one of the largest electrical companies recently stated that the only limitation to the development and extension of his industry lay in the dearth of trained men fitted to take charge of the developments which they had in sight.

The third lesson I would draw from the experience of the past is that:

III. *Research must proceed by specialization.* Research has always required concentration, but it did not formerly require intense specialization. Look, for instance, at Benjamin Franklin, at once philosopher, publisher, scientist and diplomat. Even in science he did not specialize. To quote from an admirable address by Dr. Little before the Franklin Institute:

> Among all the activities with which his busy life was crowded, Franklin undoubtedly found his greatest interest in the pursuit of science. . . . At a time when nearly everything was awaiting explanation, his focused attention ranged like a searchlight over many fields. He observed the movement of winds and developed a theory of storms. He considered ventilation and the causes of smoky chimneys and proceeded to invent new stoves. He introduced the Gulf Stream to Falmouth skippers and demonstrated the calming effect of oil on turbulent seas to officers of the British Navy at Portsmouth. From earthquakes he turned to the heat absorption of colored cloths and the fertilizing properties of gypsum. He wrote on sun spots and meteors; waterspouts, tides and sound. The kite, which for centuries had been the toy of boys, became in Franklin's hands a scientific instrument, the means to a great discovery. That its significance is, even now, not universally appreciated is shown by the recent answer of a school boy, "Lightning differs from electricity because you don't have to pay for lightning." To Franklin . . . we owe our initial conceptions of positive and negative electricity, and he was the first to suggest that the aurora is an electrical phenomenon.

But to-day each individual branch of science is larger than the entire body of science of Franklin's day. The philosopher may still contemplate the entire field of knowledge and consider its interrelationships, but no man can do creative work in the entire field. Some especially gifted men can leave the imprint of their genius in a larger variety of directions than the majority of us, but the bulk of the research of the future seems destined more and more to be carried out by people who are highly trained in specialized fields.

With this tendency comes the necessity of a balancing movement, which is my last conclusion:

IV. *Research must become more and more cooperative.* An obvious handicap and danger of specialization is that a man may not know, and in fact now-a-days can not know, even all the aspects and relationships of his own special field, however small. Hence, unaided, he becomes helpless through the very specialization which he hoped would give him power. The remedy for this danger lies in cooperation between workers in closely related branches of study.

Just as in the process of organic evolution we find increasing specialization accompanied by increasing interdependence, just as the growth of highly specialized industries has necessitated careful attention to their coordination, just so the increasing specialization in research makes necessary an increasing degree of contact and mutual assistance between scientists.

To take just one example: Though physics is the most mathematical of the sciences, the average research physicist and the average productive mathematician speak languages unknown to each other. To make advances in one field available for progress in the other there has arisen a chain of connecting links. We have the experimental physicist, the theoretical physicist, the mathematical physicist, the applied mathematician and the pure mathematician. I do not believe there is a single living scientist who could at the same time classify under all six headings. There is possibly one, named Einstein, who might classify under five, and there are very few who could classify in four of these groups.

How can this essential cooperation best be brought about? Certainly no single formula will serve to solve the whole problem, and no solution will be easy because research, to be effective, must be independent as well as coordinated, and these are two almost antagonistic features. Several solutions may, however, be suggested as of proven value and worthy of encouragement.

One of these is the encouragement of research in the so-called border-line fields such as mathematical physics, physical chemistry, biophysics, biochemistry, etc. Not only are such coordinating studies necessary, but they are, in my opinion, the most fruitful fields of investigation. Nature herself is not divided into a physical world, a chemical world, a biological world; she is a unit. These artificial distinctions have been introduced for convenience and because of our inability to see the whole field at once. They have resulted in rapid development in the particular direction and by the particular method of each of the sciences, whereas work in the border-line fields has lagged behind. It is as if the whole field of knowledge were originally a desert. When the rain fell on this desert, the water flowed off in streams and rivulets, each digging its channel deeper and deeper but leaving the intervening space relatively untouched. The best place to dig is now in these intervening spaces.

The necessity of coordinating border-line work is recognized by such far-sighted organizations as the General and International Education Boards and the Rockefeller Foundation which have, for example, supported the great system of National and International Research Fellowships, one of whose guiding principles is the stimulation of research in the border-line fields. In our universities further facility and encouragement should be given to men to prepare for work in these directions.

Another solution can advantageously be advanced by wise administration of the universities. There seems to be a wide-spread, but ill founded, feeling that all departments of a university should be developed together and kept closely abreast. Perhaps this relieves the administration from embarrassment, but I venture to suggest (though the suggestion is not new) that this is not sound educational policy except for an ideal institution which has unlimited resources. Such a policy dissipates effort, and if every institution followed it we should have the spectacle of a great many universities all very much alike and all with struggling, mediocre departments. Much more effective in advancing knowledge as well as in bringing distinction to the university is the policy of supporting to the available limit certain departments selected because of their already outstanding character, or because of the traditions and purposes of the university, or for any other reason. If these favored departments are chosen in a coordinated group, then the university becomes an active center for the development of that field and the promotion of cooperative effort. For example, one institution may choose to give *particular* facilities for advanced work in classics and languages, another in historical, economic and social sciences, another to physical and biological sciences, etc. If we were to examine the record of those universities of limited endowment which have nevertheless been preeminent in the life of the country, we should find that they attained this preeminence through concentration of effort. The words "To him that hath shall be given" apply here as well as elsewhere.

Through concentration of effort in a coordinated group of departments, a university has the opportunity not only to correct the dangers of over-specialization, but also to take a strategic position in fulfilling its obligations to society.

Much can also be done to promote cooperation and coordination through actual methods of organization. This has been strikingly demonstrated in some of the big industrial research laboratories, from which the output has greatly exceeded the individual capacities of the research workers and has been achieved only by coordination of effort. Such organization requires a very wise and far-visioned director who can visualize the big objectives and steer through the mass of petty details which must be worked out in order to attain them.

In a university, where the number of workers is much smaller than in a big industrial laboratory, such army-like organization does not appear feasible or probably desirable. Much is being done voluntarily by scientists themselves in dividing up particular

fields for cooperative investigation, and this is being fostered by research committees of organizations like the National Research Council. However, there is another direction in which more effective organization is possible within the universities themselves!

Departments of a somewhat more flexible nature than those to which we are accustomed and which could, more than now, be built around one or two outstanding men in the department, could give these men an opportunity for organization and concentration of effort which is now rarely possible. This would, of course, require careful selection of men. In this matter of organization of departments around the most productive and outstanding men, of taking for granted that they will have research assistants to increase the efficiency of their labors, of selection and recognition of men on the basis of merit and promise rather than seniority, and of wise procedure in the selection of men to fill important posts, America is far more backward and bound by tradition than are those European countries in which scientific achievements have been most rapid. It may surprise you, for instance, as it did me, to learn that in America, the land of wealth and opportunity, there is no university which is able to offer a salary equal dollar for dollar to salaries which universities even in war-ridden Germany will offer to secure the outstanding men. As is the tendency in other things American, our tendency to standardize, which is so useful in some directions, is interfering with our ability to recognize, secure and do our best. This situation in our universities is, I believe, a grave one if we set as our ideal the best possible achievement.

In these remarks I have attempted to suggest some of the accomplishments and opportunities of research and to indicate some of the directions in which we may hope to bring about even more fruitful service of science to society in the future.

This, gentlemen, is the situation. It is a situation that calls for serious thought and constructive action. The things which I have been able to say are not new, but I sincerely hope that you may find in them reasons sufficient to enlist your sympathy and active support of any movement which has for its purpose the better service of science to our country and to humanity.

KARL T. COMPTON

PRINCETON UNIVERSITY

SOME PROBLEMS IN BOTANICAL CLASSIFICATION[1]

THE systematic botanist (or taxonomist as he is often called) has a double duty. First, he must give each apparently new plant a name which other workers in plant science may use in describing their experiments with the plant or in drawing conclusions regarding plant distribution and so on. This name must designate the plant's relationships with approximate accuracy and is based on a close study of gross morphological characters. By that I mean such characteristics as shape of leaves, color of flower, number of flower parts and their arrangement—in fact such physical properties, to use a chemical phrase, as can be determined fairly readily with the naked eye or by aid of a small lens. The degree of resemblance of the total of these characters, and especially as regards those of the flower and fruit, between plants has served as the criterion in judging the degree of relationship of the plants. This type of taxonomic work will probably always be based on the same method. It is worth noting here that there still are enormous areas of the earth's surface, the vegetation of which remains comparatively unknown despite the flood of taxonomic publications during nearly two centuries, and these areas offer an attractive field for what I may call this "preliminary naming."

Second, he must revise his conceptions of plant groups in accordance with the progress of work in other botanical fields and such discoveries as are constantly being made that tend to show more clearly the exact relationships of the plants involved.

Of chief interest to us is this second phase—namely, the correlation of taxonomic work with other botanical work and the revision of opinions that necessarily results from many-sided, intensive investigations. (The preliminary naming of the cultivated fruits and vegetables of the north temperate zone has long since been completed.) Taxonomists have perhaps been a little slow in the utilization of other workers' results; but the last decade has seen started a considerable number of new lines of systematic investigation based on the work of plant physiologists and chemists, geneticists and cytologists.

It is, I believe, the correlation between the work of the geneticist and cytologist and that of the systematic botanist that offers the most promising line of attack on the age-old problem of clearing up the relationships of cultivated plants. This problem is of vital interest to us at this station.

In the Division of Horticulture, we have been particularly interested in fruits and vegetables, and, at present, some typical questions of considerable moment are: What are the true species of apple—or, in other words: What are the fairly stable natural groups of the genus Malus? What species are we working with in our attempt to breed better fruits? What are the characteristics of each of these entities? As you may readily perceive, the answers to these

[1] Extract of address before the June staff meeting of the New York State Agricultural Experiment Station, Geneva, N. Y., June 6, 1927.

questions are fundamental to a clear portrayal of breeding problems, and if these answers can be made with some degree of completeness, the formulation of breeding experiments with apples will proceed with greater facility and with increased probability of reaching favorable results. The same questions may be asked in regard to each group of cultivated plants and will have the same bearing on experimental work with them.

This taxonomic problem we are now attacking by a new method which has already shown promise in the case of the allied genus of Roses. The preliminary announcement of this work by Hurst just four years ago constitutes the first attempt to untangle a complex group of cultivated and wild forms by reference to cytological evidence.

At this point it may elucidate the situation somewhat if I review briefly Hurst's discoveries in Roses.

The inception of his work seems to have been more or less accidental. He says that one day while comparing the taxonomic characters of certain species of roses at Kew, he was struck by the fact that a species he knew to have 28 chromosomes showed the combined characters of two distinct species having 14 chromosomes each, while one containing 42 chromosomes showed the combined characters of three distinct species of the smaller number of chromosomes, and one with 56 showed the combined character of four of the others.

Further investigation of about 400 forms of Rosa have revealed 5 distinct species of 14 chromosomes each and have shown about 50 different taxonomic characters associated with each. Hurst thinks there may result over 200 commutations and permutations of these characters, and he has identified a large number of these.

Now to consider some of the possibilities arising from the uniting of germ-cells of different constitution—that is, of those containing chromosomes of different make-up, or of different numbers. It seems from such evidence as is at present available that a union of germ-cells will not take place if their constitution is too divergent. However, it also seems patent that, provided the germ-cells are very largely similar in content, a union may take place even though the number of chromosomes differ. For example, germ-cells of a species of Rose containing 14 chromosomes will sometimes unite with germ-cells containing 21 chromosomes (of another species of Rose, manifestly). In the subsequent body or somatic division of cells (as has probably already suggested itself to you) the 35 chromosomes may not divide evenly; and certainly in reforming germ-cells there can be no even division of an odd number.

The plants which arise from such combinations of germ-cells and all their descendants naturally vary a great deal in their characteristics. It is my personal belief that 99 per cent. of the difficulties which confront both taxonomists and plant breeders are due to these plants whose exact constitution can not be predetermined nor indeed determined with at all complete accuracy by use of any of the older methods.

With the foregoing considerations in mind I started this spring to attempt to unravel the species of apple by aid of such cytological and genetic evidence as might be obtainable. Dr. Bernhard Nebel, of Halle a. S., kindly took upon himself the cytological work involved. There has been almost no cytological work done on the so-called species or hybrids of Malus, nor indeed on apples at all, except for a few recent papers on the relation of chromosomes to fertility of pollen.

It is too soon to speak with much confidence as to what has been found or as to what bearing these discoveries may have on the problem; and what I have to say now may upon careful study have to be modified considerably. However, these seem to be the results and their significance at present.

Several of the plants which I believe to be good species, that are fairly stable associations of genetically closely allied plants, show 14 chromosomes in the nucleus, others show 28. These plants also show nearly regular nuclear divisions. This would seem to indicate that apples may show (as roses undoubtedly do) that good fundamental species will have either 7 or some multiple of 7 as the basic number of chromosomes. Whenever we find, as in the case of *Malus Scheideckeri,* a variable number of chromosomes and very irregular division, I think we may assume that we are not dealing with a good species but with one of the variable types of hybrids. Our work is not yet far enough along to discuss further results.

It may be interesting, however, to point out some purely speculative possibilities.

If 7 pairs of chromosomes is the fundamental number in apples, then there are probably several good species containing sets of 7 pairs each possessed of somewhat different characteristics. A union of germ-cells between two such species might give rise to another good species and undoubtedly has done so. Moreover, apple species of 21, 28, or 35 chromosomes may exist—according to one investigator—and combinations between these have probably produced other good and valid species. *But* a combination between a species with 14 chromosomes and one with 21 might and probably has produced a very variable series of offspring.

Just how far these possibilities may fit with the facts as shown by results of experimental work done on apples is now a part of the problem.

I refer to "experimental work done" because for a number of years it will be necessary to utilize all of the data obtainable from experiments that have been tried with other aims in view. This is due to the facts, first, that no attempts have as yet been made by geneticists to breed apple species for the specific purpose of determining their taxonomic position in relation to each other; and, second, that apples are so slow in maturing that it takes half a life time to get three generations. Perhaps I should have put the statement the other way about and said that man lives so short a time that he can manage to see only 3 or 4 generations of apples in his life. Of course, these statements also apply, with more or less aptness, to other perennial fruit crops. The vegetable crops offer a more satisfactory field in point of their shorter individual lives and greatly increased number of generations per given time. These, too, we hope to investigate in the same way.

This whole situation leads much farther than the naming of types of plants. It promises, I believe, a tremendous advance in taxonomic accuracy for the very reason that it may give us a clear-cut definition of a species. Heretofore, a species has always been a concept and no two taxonomists have agreed entirely in their specific concepts. The more variation there is in a group of plants, the greater the diversity of concepts existing among specialists on that group. In cultivated plants, because of the tremendous variations induced by men by selection, crossing, and removal of natural competition and other difficulties in what might be called the "normal life struggle" of the plant, this diversity has been increased many fold. The remarkable thing, it seems to me, is that there exists as much agreement in specific concepts as there is. This new method seems to promise an approximation, at least, to mathematical accuracy. Given a definite number of pairs of chromosomes and a perfectly regular method of cell division allied with a distinct group of other morphological characteristics and we have a good species. Given an indefinite number of chromosomes and irregular division, we haven't a good species but a hybrid of sorts, characterized in every case investigated thus far by considerable variation in gross morphological characters.

If, as I have assumed, the greater part of the difficulties lie in the varying concepts regarding those plants which are not *good* species according to my definition of a moment ago, then a clarification of the status of those plants by reference to their cytological behavior will go a great ways in stabilizing nomenclature of plants in general. Perhaps, I should have said "higher plants," for I fear this investigation of chromosomes will be of little use to the bacteriologists who have yet to find such things in their whole category of delightful pests. However, the bacteriologists by their use of physiological reactions have gone further, I believe, in attaining accuracy in delimitation of related forms than the systematic botanists who deal with seed plants and trust to their eyes alone to determine differences and likenesses.

This may lead to some difficulties in terminology involving among other things the uses of such words as hybrid, cross, variety, form and species. It seems to me that the term hybrid will have to be held merely for those results of crossing in which the ensuing nuclear divisions exhibit irregularity of behavior coupled with varying degrees of sterility. And it is to be noted that so far as known at present some degree of sterility is *always* associated with this irregular chromosome behavior. It seems likewise evident that when the result of a cross is an organism with perfectly regular nuclear division and complete potential fertility, it must be reckoned a good species regardless of whether it has been found wild or is known only under cultivation.

I believe I have exhausted my time and possibly your interest. My own interest, I have to confess, is white hot. I feel that we are in view of some striking advances in taxonomic work and it is a rare pleasure to be in at the very beginning of the adventure.

G. P. VAN ESELTINE
AGRICULTURAL EXPERIMENT STATION,
GENEVA, N. Y.

SCIENTIFIC EVENTS

THE INTERNATIONAL CONGRESS OF ENTOMOLOGY

THE Fourth International Congress of Entomology is to be held in August, 1928, at Cornell University, Ithaca, N. Y. Previous congresses have met at Brussels (1910), Oxford (1912) and Zurich (1925). Every important interest—educational, scientific and economic—will be provided for in the program. Invitations have been forwarded through the state department to foreign governments to send representatives and later invitations will also be sent to the individual entomologists. A program will be arranged in which some of the leading entomologists of the world will take part. It is planned that in the forenoons throughout the week papers of general interest are to be read before all members of the Congress. In the afternoon sections will be formed dealing with (1) taxonomy, distribution and nomenclature; (2) morphology, physiology and genetics; (3) ecology; (4) medical and veterinary entomology; (5) economic entomology with its subdivisions relating to forest, fruit, vegetable and cereal insects, bees, insecticides

d appliances. According to the number of papers nounced, each section may be subdivided or several tions may be united. Time will be arranged for all-day visit to the Geneva Experiment Station ere the forenoon will be spent in looking at the hibit of spraying machinery and insecticides, in amining the methods and machinery used in con-lling the European corn borer, and in observing a monstration of airplane dusting. The afternoon ill be devoted to a general program. In addition, summer meeting of the New York State Horti-ltural Society will be held at Geneva on the same y, which will serve to give the visiting foreign tomologists more ideas of the general interest of merican farmers in entomology. Certain afternoon cursions will also be made to nearby places of ento-logical interest, while immediately after the meet-gs some general excursions are planned to Niagara lls, to entomological museums of eastern cities, and the laboratories of the U. S. Bureau of Entomology voted to the study of the gipsy and browntail oths, the corn borer and the Japanese beetle.

THE WALTER RATHBONE BACON SCHOLARSHIP

UNDER the terms of the will of the late Virginia Purdy Bacon, of New York, the Smithsonian Institution receives a bequest to establish a traveling scholarship as a memorial to her husband, Walter Rathbone Bacon.

The secretary of the Smithsonian Institution has established rules which are to regulate the award of the Walter Rathbone Bacon scholarship for the study of the fauna of countries other than the United States of America. The amount available is the interest on the capital invested (about $3,000 a year), the incumbent to hold the scholarship not less than two years.

Applications for this scholarship, addressed to the secretary of the Smithsonian Institution, should be submitted not later than January 1, 1928. The application should contain a detailed plan for the proposed study, including a statement as to the faunal problems involved; the reasons why it should be undertaken; the benefits that are expected to accrue; the length of time considered necessary for the carrying out of the project; the estimated cost, and the scientific and physical qualifications of the applicant to undertake the project.

The scholarship will be awarded for a term of two years. If at the expiration of the term it is desired to extend the time, the incumbent shall make application a sufficient time in advance, accompanied by a statement as to the necessity for such extension.

All collections, photographs, records and equipment become the property of the institution.

The incumbent shall not engage in work for remuneration or receive salary from other sources than the institution or its branches during the period of occupancy of the scholarship.

THE LINTHICUM FOUNDATION PRIZE

THE faculty of law of Northwestern University, administering the income of the Charles C. Linthicum Foundation, announces that the sum of one thousand dollars and a bronze medal, as a first prize, and two sums of one hundred dollars each, as second prizes, with honorable mention, will be awarded to the authors of the best essays or monographs submitted by March 1, 1929, on "Scientific Property," *i.e.*, the extension of the patent or copyright laws so as to recognize a right, in the discoverer of a scientific principle, to some share of the profits that may accrue to an inventor who makes use of that discovery to devise an "art, machine, manufacture or composition of matter" (as the statute defines it) and thus obtains a patent. The law has hitherto not recognized such a right.

The subject of the award in 1927 was "The Law of Radio-Communication," and the prize was awarded, on June 16, 1927, to Stephen Davis, Esq., member of the bars of Oklahoma and New York, and formerly solicitor to the United States Department of Commerce.

The present offer was originally opened only to members of the legal profession in the United States or Canada, but has now been enlarged to include all countries of the world.

The award will first be made public in June, 1929, on the occasion of the annual meeting of the Alumni Association of the Law School.

Further information may be obtained by addressing the Linthicum Foundation, Northwestern University Law School, Chicago. The work submitted may be one already published in print at the time of submission. Manuscripts submitted must be typewritten on paper of size of legal cap or typewriter or commercial note, and in the English language. A work submitted in French, German, Italian or Spanish may be examined, at the discretion of the faculty; but, if awarded the first prize, it must be translated into English for publication, at the expense of the author.

THE ENGINEERING INDEX SERVICE

PLANS have been completed in a comprehensive scheme for indexing the engineering literature of the world, to be initiated the first of the year, with the new weekly engineering index service of the American Society of Mechanical Engineers.

This task is so extensive as to include the preparation of index items for the 1,500 technical publications

of the world appearing in 17 languages in 37 countries which are received in the Engineering Societies Library. The index items will be printed on cards and mailed weekly to the subscribers to the new service. This project was authorized by the American Society of Mechanical Engineers' council at its St. Louis meeting on October 8, 1927, as an important extension of the *Engineering Index* in its service to research and the development of industry and to bring to every one, not only in the United States but to any in the world, the storehouse of knowledge appearing in the technical literature of the world.

The American Society of Mechanical Engineers has published the *Engineering Index* since 1919, when it was taken over from the *Engineering Magazine*. During the eight years this book has been published by the society, its volume of material has increased from approximately 8,000 items annually to 16,000. These were first published monthly in *Mechanical Engineering* and at the end of the year collated in an annual volume. In the limited scope of a book of this kind it is possible to give only an incomplete service, while the modern needs of industry demand complete information about published technical material throughout the world and require it promptly. Accordingly, the new scheme was developed to meet this demand.

This service is made possible by the cooperation of the library board of the United Engineering Society Library and, in addition to the augmented technical staff of the society, the especially trained group of librarians who have on file the magazines to be indexed and who are prepared to furnish photostat copies and translations of foreign articles to those requiring them.

The new service will be under the general supervision of the committee on publications of the society, of which Ralph E. Flanders is chairman, and the finance committee, under the chairmanship of H. V. Coes. Professor Carlos deZafra, of New York University, will occupy the post of director of *The Engineering Index*.

THE COMMISSION FOR RELIEF IN BELGIUM FELLOWSHIPS

THIRTY-THREE Belgian advanced and graduate students will study in America during the whole or a part of the year 1927–1928 as holders of fellowships under the auspices of the Commission for Relief in Belgium Educational Foundation, 42 Broadway, New York. Three American fellows are at present in Belgium for a full year of advanced work.

The distribution of fellows in the field of science and their subjects of post-graduate study are as follows:

Harvard University: in neurology, Dr. Jacques DeBusscher.

Massachusetts Institute of Technology: in metallurgy, Marcel Allinckx; in electrical engineering, Leopold De Beer; in automotive engineering, Paul DePermentier; in hydraulic engineering, John L. Eeman; in aeronautics, Frits P. Malschaert.

Stanford University: in electrical engineering, Carl L. DeBrouwer (honorary fellow); in economic geology, Robert DeStrycker; in geology, Joseph Donnay; in electrochemistry, Pierre van Rysselberge.

University of California: in physiology, Dr. Lucie Brouha; in chemistry, Yvonne Lieben; in bacteriology, Dr. Jeanne Lommel.

Columbia University: in psychology, Dr. Simone Ghisland; in radio-activity, Dr. Suzanne Simon.

Western Reserve University: in pharmacology, Dr. Corneille Heymans; in physiology, Dr. Pierre Rijlant.

Yale University: in chemistry and hygiene, Dr. Francois Schoofs; in embryology, Dr. Ernest Van Campenhout.

California Institute of Technology: in physics, Willy Uyterhoeven.

Carnegie Institute of Technology: in steel industry, Joseph DeWez.

Cornell University: in thermodynamics, Albert DeSmaele.

Rockefeller Institute: in respiratory diseases, Dr. Pierre Denys.

Special Investigations and Travel: in bacteriology, Professor Richard Bruynoghe; in Greek archeology, Herbert Philippart.

The Americans in Belgium include Dr. George Sauté, of Harvard University, in mathematics.

Each fellowship provides full traveling expenses, payment of tuition fees and a limited stipend for living expenses. This fellowship exchange began in 1920 and, in all, 73 American and 218 Belgian fellows have been appointed. The fellowships were established to commemorate the war-time relief work of the Commission for Relief in Belgium and to promote the exchange of intellectual ideas and good-will between Belgium and America.

SCIENTIFIC NOTES AND NEWS

DR. THOMAS BARBOUR, curator of reptiles and amphibians in the museum of comparative zoology at Harvard University, has been appointed director of the museum to succeed Dr. Samuel Henshaw, who recently resigned after serving for twenty-three years.

DR. GEORGE K. BURGESS, director of the U. S. Bureau of Standards, has returned from Paris, where he officially represented the United States at the Seventh International Conference on Weights and Measures.

DR. HAVEN EMERSON, professor of public health administration in the College of Physicians and Surgeons, Columbia University, is to give a series of lectures during the week of November 28 to Decem-

r 3 in London, at the invitation of the health section f the League of Nations. The topics of his lectures ill be, "Epidemiology in Non-communicable Dis-ases," "The American Program for the Prevention f Heart Disease" and "Diabetes Mortality from the ublic Health Point of View."

ON the occasion of the formal opening of the new edical school and clinics at the University of Chicago October 31 the honorary degree of doctor of ience was conferred upon Dr. Frank Billings, meritus professor of medicine in the university; Dr. ufus Cole, director of the hospital of the Rocke-ller Institute; Dr. William S. Thayer, professor meritus of medicine at the Johns Hopkins Univer-ty, and Dr. Karl Landsteiner, of the Rockefeller stitute.

THE International Urological Congress, meeting in russels, conferred upon Dr. Edwin Beer, visiting rgeon at Bellevue Hospital, New York City, the rst award of its gold medal for the introduction a new method of curing bladder tumors by high equency currents.

THE Baly medal of the Royal College of Physicians, ngland, was presented on October 18 to Dr. A. V. ill, Foulerton professor of physiology at Univer-ty College, London.

THE Compton medal of the Institution of Auto-obile Engineers has been awarded to H. R. Ricardo r his paper entitled "Some Notes on Petrol-Engine evelopment."

AT the annual meeting of the Royal Society of dinburgh, held on October 24, the following officers d members of council were elected: *President,* Sir mes Ewing; *General secretary,* Professor R. A. mpson; *Treasurer,* Dr. J. Watt; *Curator of Library d Museum,* Professor D'Arcy W. Thompson; *Coun-llors,* Professor G. Barger, Mr. J. Bartholomew, rofessor C. G. Darwin, Professor D. Waterston, Mr. W. Peck, Dr. J. Ritchie, Professor R. Stanfield, Dr. L. Turner, Dr. G. W. Tyrrell, Professor J. H. Ash-rth, the Hon. Lord Constable, Professor E. Taylor nes.

SIR WILLIAM LARKE, director of the National Fed-ation of Iron and Steel Manufacturers, has been ppointed a member of the advisory council to the mmittee of the Privy Council for Scientific and dustrial Research.

PROFESSOR A. L. KROEBER, chairman of the depart-nt of anthropology at the University of California, s been named faculty research lecturer for 1928. e faculty research lecturer is selected each year by faculty committee from among those connected with the university who have contributed most to science during the previous year, or who have concluded some particularly interesting piece of work.

DR. WILLIAM TOWNSEND PORTER, professor of comparative physiology in the Harvard Medical School, has been made professor emeritus.

H. A. MARMER, of the U. S. Coast and Geodetic Survey, has been appointed a member of the committee on tides of the International Geodetic and Geophysical Union.

DR. GEORGE GRANT MACCURDY, of Yale University, has been chosen as one of the seven foreign patrons of the newly established Prehistoric Society of Morocco. Dr. MacCurdy returned to New Haven in October after a summer in Europe directing the work of the American School of Prehistoric Research.

JOHN A. COMSTOCK has been appointed acting director of the Los Angeles Museum and will serve in this capacity during the absence, in Europe, of William Alanson Bryan.

GEORGE B. WATKINS has resigned his position as instructor of chemical engineering at the University of Michigan, to accept a position as chemical engineer in the research department of the Libbey-Owens Sheet Glass Company, Toledo, Ohio.

DR. CLARENCE E. KOBUSKI has been appointed assistant at the Arnold Arboretum of Harvard University.

DR. GEORGE E. BREWER, professor emeritus of surgery at Columbia University, sailed from the United States on October 15 for Europe, where he expects to spend the year in anthropological study.

DR. IMMANUEL FRIEDLANDER, director of the Vulcanological Laboratory at Naples, Italy, is visiting the United States.

DR. DAVID LINDER, instructor of botany in Washington University and mycologist to the Missouri Botanical Garden, after a year's leave of absence in Africa, has returned to this country and has taken up his work in Washington University and the garden.

DR. LEOPOLD HERRY, director of the Central Electrique des Flanders at Langerbrugge, and Dr. I. J. Moltkehansen, well-known Norwegian electrometallurgist, and Consul Honaire de France, Brussels, who accompanied the American Electrochemical Society on its industrial trip through the Northwest and later visited many important universities and research laboratories in the East, have sailed for Belgium.

DR. FELIX DEUTSCH, head of the Allgemeine Elektrizitäts-Gesellschaft, sailed for home on November 2, following a short visit to the United States.

Dr. Sven Ingvar, docent in neurology at the University of Lund, Sweden, gave three lectures on the Herter Foundation at the Johns Hopkins University on November 3, 4 and 5. His subjects were "The Cerebellum: its Structure and Function" and "Pathogenesis of Argyll-Robertson Phenomenon."

Dr. Raoul Blanchard, professor of geography at the University of Grenoble, offered a series of lectures on "Human Geography of High Mountains" and a series on "Urban Geography" at the school of geography in Clark University during the month of October. Dr. C. F. Marbut, director of the U. S. Bureau of Soils, is at present lecturing at the school on the "Origin and Development of Soils in the United States." Dr. Frederick Starr, professor emeritus of anthropology at the University of Chicago, will give a series of four illustrated lectures on Japan at the university during the week beginning November 14.

Dr. I. Snapper, professor of pharmacology and general pathology, University of Amsterdam, addressed the University of Wisconsin Medical School on November 1 on "Non-excretory Functions of the Kidney."

The Westbrook lectures for 1928 of the Wagner Free Institute of Science, Philadelphia, will be given in March by Dr. Dayton C. Miller, of the Case School of Applied Science, on "The Science of Musical Sounds." Four lectures will be given.

Dr. Frank C. Mann, director of the division of experimental surgery and pathology at the Mayo Clinic and Mayo Foundation, Rochester, Minn., will deliver the second Harvey Society lecture at the New York Academy of Medicine on Friday evening, December 9. His subject will be "The Relation of the Liver to Metabolism."

Professor James Kendall, of the department of chemistry, Washington Square College, New York University, addressed the Philadelphia section of the American Chemical Society on October 20, on "The Abuse of Water." The same lecture was delivered before the department of chemistry of Mt. Holyoke College, on October 28.

Dr. Thorne M. Carpenter, chief chemist and vice-director of the Nutrition Laboratory of the Carnegie Institution of Washington, delivered a lecture at the University of Missouri on October 31, under the joint auspices of the Society of Sigma Xi and the American Chemical Society on the subject "The Carnegie Nutrition Laboratory and its Recent Studies in Metabolism."

H. E. Howe, editor of *Industrial and Engineering Chemistry,* during the month of October spoke before the Southern Appalachian Power Conference in Chattanooga, Tenn.; the Institute of American Meat Packers in Chicago, Ill.; the Kiwanis Club of York, Pa.; the Chemists' Club of Chicago, and the Milwaukee section of the American Chemical Society.

Dr. W. F. G. Swann, formerly professor of physics at Yale University and now director of the Bartol Research Foundation of the Franklin Institute, delivered the founders' day address at Swarthmore College on October 29.

The Thomas Hawksley lecture of the British Institution of Mechanical Engineers, on "Application of X-rays to the Study of the Crystalline Structure of Materials," was delivered by Sir William Bragg, on November 4.

An effort is being made by the Southwark Borough Council to commemorate Michael Faraday, who, the son of a blacksmith, was born in Southwark, England. The purpose of the council is to establish a Faraday memorial collection in the central reference library. The collection will consist of the biographies, portraits and published works of Faraday, and the best and latest books on the sciences and their applications, particularly electricity, with which Faraday's name is identified. It is purposed to establish a Faraday memorial fund, the annual income from which will be expended each year under the direction of a special committee, exclusively for the purpose in view.

Dr. William R. Orndorff, professor of organic chemistry at Cornell University, died on November 7, aged sixty-five years.

Dr. John C. Warren, professor emeritus of surgery at the Harvard Medical School, died on November 4, in his eighty-sixth year.

H. M. Taylor, F.R.S., senior fellow and former mathematical lecturer of Trinity College, Cambridge, distinguished by his contributions to mathematical science, died on October 16, at eighty-five years of age.

Dr. J. W. Mollison, formerly inspector-general of agriculture in India and who was the first head of the Imperial Agricultural Research Institute at Pusa, died on October 4, aged seventy years.

Professor Max von Grüber, president of the Bavarian Academy of Sciences and formerly head of the department of hygiene at the University of Munich, died on September 16.

Professor Hans Leo, head of the department of pharmacology at the University of Bonn, has died aged seventy-four years.

The United States Civil Service Commission states that the position of professor of chemistry at the

ienic Laboratory, Public Health Service, Treasury epartment, is vacant, and that, in view of the imortance of the position in the field of medical, chemal and public health research, and to insure the ppointment of a thoroughly qualified man for the ork, the qualifications of candidates will be passed pon by a special board of examiners, composed of r. G. W. McCoy, director of the Hygienic Laboraory; Dr. Charles L. Parsons, chemical engineer, Mills uilding, Washington, D. C.; Dr. Julius Stieglitz, ead of the department of chemistry, University of hicago; Dr. Charles H. Herty, Chemical Foundation, ew York City; Dr. Reid Hunt, Harvard Medical chool, and A. S. Ernest, examiner of the United tates Civil Service Commission, who will act as chairan of the committee. The duties of this position will onsist of planning, conducting and supervising reearches in the field of chemistry as it relates to the ublic health. The incumbent will have full charge, s chief, of the Division of Chemistry and as such will e responsible for the administration, personnel and cientific work of the division. The entrance salary f this position is $6,000 a year.

THE Maryland section of the American Chemical ociety will act as host to the Washington, Philadelhia, Virginia, Delaware and South Jersey sections t an intersectional meeting to be held at the Univerity of Maryland on November 26. At that time the niversity's newly-erected chemistry building will be edicated. The new laboratory is a four-story structure, erected at a cost of $250,000. Additional funds or the purchase of furniture and equipment have been onated by friends of the institution as follows: *Organic,* Dr. H. A. B. Dunning, of Hynson, Westcott nd Dunning; *Analytical,* Dr. Samuel W. Wiley, of Wiley and Company, Inc.; *General Chemistry,* Dr. and Mrs. M. L. Turner; *Physical,* Dr. Alfred R. L. Dohme, f Sharpe and Dohme; *Colloid,* Mr. C. G. Campbell, of the Kewaunee Manufacturing Company; *Industrial,* The Chemical Alumni of the University of Maryland. The dedicatory exercises, to which the public is inited, will be held at 10:00 A. M. Dr. Edgar F. mith, of the University of Pennsylvania, will be the principal speaker. A special luncheon has been arranged. The afternoon session will be devoted to the ntersectional program. A series of papers have been rranged under each of the following sections: Physial and inorganic, organic and biological, industrial nd agricultural and the chemical education. Dr. C. H. Herty will deliver an address at an informal dinner hich will be held in the evening. It is essential that he committee know, not later than November 18, how many plan to be present. It is desired that those atending the meeting notify Dr. N. E. Gordon, Univerity of Maryland, College Park, Md.

ACCORDING to *Museum News* the Mid-West Museums Conference is to hold its annual meeting at St. Paul and Minneapolis on November 18 and 19. One of the features of the sessions will be the joining in of the newly formed Michigan-Indiana Museums Association. This group was organized at a meeting held at the Chamberlain Memorial Museum, Three Oaks, Michigan. Announcement of intentions to affiliate themselves with the Mid-West Conference was made by the organizers and the newly elected officers. George R. Fox, director of the Chamberlain Museum, was elected president of the new association. Edward M. Brigham, curator of the Museum of Natural History of the Battle Creek Public Schools, was named secretary.

WOMEN planning to attend the meetings of the American Association for the Advancement of Science will be interested in knowing that lodging can be obtained at the dormitory of the Peabody College for $2.00 per day. The Peabody and Vanderbilt Universities will be used for the meetings and there are eating places near at hand. Application should be made to W. N. Porter, convention secretary, Chamber of Commerce, Nashville, Tennessee.

THE fiftieth anniversary of the American Society of Mechanical Engineers will be celebrated during the week beginning April 7, 1930. This date marks the anniversary of the organization meeting of the society which was held at Stevens Institute of Technology. While plans for the proposed celebration have not been completed, the tentative arrangement provides concurrently for an international engineering congress of outstanding nature which will not only mark the achievements of the engineering profession for the past fifty years, but will point the way for future growth and development. It is expected that this fiftieth anniversary meeting will be held in Washington, D. C., thus giving it a national and international character.

AT the invitation of the American Hospital Association, representatives of eleven countries met on September 19, at the League of Red Cross Societies in Paris, to discuss an international hospital convention. The meeting was composed of representatives from the International Council of Nurses and the League of Red Cross Societies, also representatives from Belgium, Czecho-Slovakia, Denmark, France, Great Britain, Hispana-America, Hungary, Italy, Mexico, Netherlands, Sweden and the United States. At this meeting it was decided that the first International Hospital Congress should meet in the United States in June, 1929, and that the invitations extended by various European cities be taken into consideration for the second congress.

THE Henry Herbert Wills Physics Laboratory, an imposing addition to the buildings of the University of Bristol, was formally declared open on October 21, by Sir Ernest Rutherford, president of the Royal Society. The laboratory is one of the finest and best equipped in the world. It stands on the Royal Fort Estate, overlooking the city. The late Mr. H. H. Wills was the donor, his gift amounting to £200,000. The opening ceremony took place in the main lecture theater of the laboratory, and the chancellor of the university, Lord Haldane, who presided, was supported by officers of the university and academic staff and by a group of distinguished physicists who visited Bristol for the occasion. Subsequently the chancellor admitted to the degree of doctor of science *honoris causa* Professor Max Born (Göttingen), Sir William Bragg (Royal Institution, London), Professor A. S. Eddington (Cambridge), Professor Alfred Fowler (Imperial College of Science and Technology, London), Professor P. Langevin (Paris) and Sir Ernest Rutherford (Cambridge).

A FIVE years' program of scientific studies in the physical chemistry of steel making to be carried out by the Carnegie Institute of Technology, the U. S. Bureau of Mines and an advisory board of Pittsburgh steel executives and metallurgists has been launched. Twenty-six steel companies located east of the Mississippi River will cooperate in the work. Pledges of support in undertaking the research program were given by 65 representatives of the 26 cooperating steel companies at a dinner given for them and members of the advisory board by President Thomas S. Baker at the Carnegie Institute of Technology on October 19. The dinner meeting was preceded by inspection in the afternoon of the metallurgical laboratories of the Bureau of Mines and the laboratories of the department of metallurgy and the bureau of metallurgical research at the Carnegie Institute of Technology. Speakers at the meeting included Dr. Thomas S. Baker, president of the Carnegie Institute of Technology; Dr. John Johnston, director of the department of research and technology, United States Steel Corporation; Scott Turner, director, U. S. Bureau of Mines, and Dr. C. H. Herty, Jr., head of the ferrous metallurgical section of the U. S. Bureau of Mines, who gave a progress report on the cooperative research.

THE Captain Marshall Field Brazilian Expedition, which left Chicago in June, 1926, ended with the return to the Field Museum of Natural History on October 27 of Colin C. Sanborn, the last of its members to remain in the field. The zoological section of the expedition originally included in its personnel, besides Mr. Sanborn, George K. Cherrie as leader, Mrs. Marshall Field, Curzon Taylor and Karl P. Schmidt. These other members returned at various times after completing the particular branches of the work they were interested in. They obtained for the museum 4,333 specimens of mammals, birds, reptiles, amphibians, fishes, insects and other creatures. The original expedition also had a botanical division in charge of Dr. B. E. Dahlgren, acting curator of botany, and a geological division headed by Henry W. Nichols, associate curator of geology, which obtained valuable collections for those departments of the museum.

UNIVERSITY AND EDUCATIONAL NOTES

AN anonymous gift of $150,000 has been made to Princeton University for the establishment of a chair of geography to be known as the Knox Taylor professorship.

THE Harvard Medical School has been made the beneficiary of a sum amounting to $150,000 by the will of the late William A. Purrington. The bequest is made "for research work in the field of medicine with special reference to the application of medical knowledge to the department of dentistry."

DR. ERNEST SHAW REYNOLDS has been appointed professor of plant physiology in the Henry Shaw School of Botany of Washington University and physiologist to the Missouri Botanical Garden. Dr. J. M. Greenman, curator of the herbarium, Missouri Botanical Garden, and professor of botany in Washington University, has been placed in charge of graduate work in the Henry Shaw School of Botany. Dr. Roland V. L. La Garde has been appointed research assistant on the staff of the Missouri Botanical Garden.

RALPH L. SHRINER has resigned his position as associate in research at the New York Agricultural Experiment Station, Geneva, N. Y., to accept an assistant professorship in organic chemistry at the University of Illinois.

DR. CHARLES L. SWISHER, professor of physics at North Dakota College, and Dr. John E. Pomeroy, formerly of Bethany College, have been appointed assistant professors in the department of physics at Yale University.

DR. W. F. WENNER and Dr. L. A. Brown have been appointed to assistant professorships in zoology at the State University of Iowa.

DR. MELVILLE J. HERSKOVITS, lecturer in anthropology at Columbia University, has joined the faculty of Northwestern University.

DR. J. DUESBERG, professor of anatomy at the faculty of medicine of Liége, who, during the war, served

in the Johns Hopkins Hospital at Baltimore, has been appointed rector of the University of Liége for the period 1927–1930.

PROFESSOR H. VILLAT, of the University of Strasbourg, has been appointed to the newly established chair of the mechanics of fluids at the Sorbonne.

DISCUSSION AND CORRESPONDENCE

THE TILDEN METEOR, AN ILLINOIS DAYLIGHT FALL

ON the afternoon of July 13, 1927, at about 1:00 P. M. central standard time, a stony meteor, hereafter referred to as the Tilden meteor, fell near Tilden, Illinois, about forty-five miles southeast of St. Louis, Missouri. The meteor fell in an area roughly two by seven miles, and four stones have been recovered, three of which weigh, respectively, one hundred and ten, forty-six, and nine pounds. The fourth is a small piece weighing a fraction of a pound.

The meteor came from the southeast, its path being inclined at an angle of perhaps fifty degrees to the horizontal, and with a velocity equal to, or slightly in excess of, the parabolic. Its brilliancy was such that at a distance of more than a hundred miles it appeared as "a piece falling off the sun." At a height of fifteen or twenty miles it burst, showing green and then purple, and after a second bursting was invisible to persons at a distance. A cloud of smoke was visible near the point of fall, but the falling pieces quickly had their velocity reduced so that they were no longer luminous by daylight, and only one piece was actually seen while falling. It was seen as "a dark streak, like smoke, for an instant."

The sky was partly cloudy in the vicinity of the fall, so few there saw anything, although nearly every one was looking, after the house-shaking blasts of the detonations. Following the detonations a roar like a tornado, or an earthquake, rolled to the southeast and died away in the distance. The meteor travelled with a velocity greater than that of sound, so the roar from the more distant portions of the path was heard after the detonations of the bursting in the nearer portion. This helped in evaluating the stories of the few who saw anything, for every one heard the sound rolling toward the southeast and assumed the meteor was travelling in that direction. The stones were actually seen to fall, and the smoke to roll, in the opposite direction.

The falling stones made a hum like an airplane flying high. The two larger stones could both be heard over considerable territory and at one place five men were out in a group straining their eyes to see an aviator who "flew over and passed out of hearing in the northwest, then came back flying much lower and landed a little to the north of the group."

The three larger pieces were heard to strike, the largest a few seconds after the blasts, the forty-six-pound piece "perhaps three minutes after," and for the nine-pound piece we have two careful estimates, "three to five minutes" and "five to eight minutes." The fact that for even the largest stone the thud of striking the earth was heard after the detonations of the bursting meteor shows that the average velocity of the fall from the point of bursting to the earth must have been less than the velocity of sound. Since the velocity of this meteor was twenty-five to thirty miles per second in the upper atmosphere, and sound travels at the comparatively leisurely rate of a mile in some five seconds, we have a striking illustration of the tremendous resistance of the lower atmosphere to bodies travelling at high velocities.

The soil of the territory is rather a stiff clay, and it was very hard because of no rain for weeks. The largest piece struck on the edge of a field of cow-peas, and went down three feet ten inches. The forty-six-pound piece went down fifteen inches in a clover pasture. The nine-pound piece went down five inches in grass in a back yard, and the small piece was found lying on a lawn. The fall was nearly vertical at the last, the largest stone departing about six inches from the vertical in penetrating three feet ten inches. The impact in no case noticeably scattered the soil; the holes were simply driven into the ground. The nine-pound stone was easily lifted out of the hole. For the forty-six-pound piece a little digging with a pocket knife was necessary; and the removal of the one hundred and ten-pound stone required two hours' hard work for two men with spade, pick and crowbar. It was wedged "as if it were set in concrete."

The meteorites are composed of a light gray stone, and show small silvery globular aggregates, presumably of nickel-iron. The surfaces show typical pittings and a typical black crust, being blackened and pitted in fairly uniform fashion. From a preliminary study of the literature available, this fall appears to be the first recorded from the state of Illinois, and the one hundred and ten-pound stone ranks among the largest seen to fall and preserved reasonably intact. Plaster casts will be made of the larger stones of this fall.

It should be said that the information in this note was obtained by personal interview, the writer visiting people, not only in the vicinity of the fall, but more than a hundred miles from that point.

CHARLES CLAYTON WYLIE
UNIVERSITY OF IOWA

ETIOLOGY OF EUROPEAN FOUL-BROOD OF BEES

SINCE Cheshire and Cheyne investigated the cause of foul-brood of bees in England and attributed the

etiology of the disease to *B. alvei,* which is almost invariably found in large numbers in infected larvae, much work has been done to corroborate their results. In no case, however, has an isolated culture of *B. alvei* been known to produce the disease. On the other hand, G. F. White and others have refuted the claim of Cheshire and Cheyne and ascribed infection in this disease to *B. pluton.* Owing to their inability to cultivate and isolate the organism, however, their claim has remained hypothetical; for it could not be determined whether this organism was itself merely a secondary invader—as they said was *B. alvei*—or whether the infection was mixed, or whether, indeed, these organisms played any pathological rôle in the disease.

It has been the writer's fortune, however, to develop a medium admirably suitable for the growth of *B. pluton* (White). An 0.15 per cent. concentration of agar, together with certain nutrients, is employed as an enrichment medium; and a concentration of 1.5 per cent. agar for the isolation of the organism at 37° C. By this method pure cultures of *B. pluton* can be readily obtained, provided the larvae used contain a preponderance of this organism.

The writer has obtained infection in a healthy colony of black bees in four days, using as inoculum cultures of the organism derived from isolated colonies. The symptoms of the diseased larvae accorded with those observed in naturally infected larvae, and the microscopical picture was typical—*B. alvei* forms being also present, though only in small numbers. The organism has been reisolated successfully.

Morphological studies thus far suggest the identity of the two organisms. While the results in this are not yet complete, cultures of *B. pluton* have been observed to change to *B. alvei* form, resembling biologically the *B. alvei* isolated from infected larvae. This further corresponds very closely with the changes observed in brood naturally infected, where the ratio of *B. alvei* to *B. pluton* generally increases as the putrefaction of the larvae progresses, so that *B. pluton* is almost eliminated. The more conclusive substantiation of this is anticipated, and its accomplishment should lead to the demonstration of important relations between the pathogenicity of microorganisms and their life stages.

DENIS R. A. WHARTON

OTTAWA, CANADA

NOTE ON A SECOND OCCURRENCE OF THE MOSASAURIAN REPTILE, GLOBIDENS

IN 1912 (Proc. U. S. Nat. Mus., vol. 41, p. 479) the new genus and species, *Globidens alabamaensis* Gilmore, was established on a rather meager specimen from the Upper Cretaceous of Alabama. The unusual globular form of the teeth as contrasted with the pointed, sharp-cutting teeth of other Mosasaurians made this an outstanding genus on which Dollo has subsequently founded a distinct family, the Globidensidae.

Recently I have received for examination the crowns of two teeth collected from the Selma Chalk in the vicinity of Saltillo, Lee County, Mississippi, by a student of Prof. J. M. Sullivan, of Millsaps College, Jackson, Mississippi.

The crowns of these teeth show no evidence of wear and this fact, in conjunction with their relatively small size, would indicate that they were probably germ teeth which had not yet come into use. The globular form of their crowns, with wrinkled enameled surfaces, however, are in perfect accord with the teeth of the type specimen.

The fragmentary character of the specimen contributes nothing new to our knowledge of this little known Mosasaurian, but it is of interest as recording a new occurrence, and especially in definitely locating its geological occurrence as being in the Selma Chalk.

CHARLES W. GILMORE

U. S. NATIONAL MUSEUM

MORE AND BETTER ETHICS FOR SCIENTIFIC MEN

THAT the code of ethics[1] adopted at the Santa Fe meeting of the Southwestern Division, American Association for the Advancement of Science, has been found by Dr. Kempton[2] a subject for genial mirth, seems to call for comment from some other quarter than that immediately involved, the members of the Southwestern Division having, one might say, cramped their style in controversy by the adoption of Rule 4. Thus, as so often, a new law works hardship first upon the law-abiding.

Dr. Kempton, as a resident of the Atlantic coastal plain, can hardly be expected to understand the distressing conditions prevailing in scientific and educational circles in outlying provinces west of the Appalachian Highland. It is a source of deep gratification to us in the West to learn that the conditions deprecated in the resolutions mentioned are nonexistent in the East, which we have so long been taught to look to as the home of culture, truth and grace. The writer is glad to be corrected in his evidently erroneous assumption that Rule 10, for example, might, in the awkward gambols of its play-

[1] "A Code of Ethics for Scientific Men," SCIENCE, Vol. LXVI, No. 1700, pp. 103–104, July 29, 1927.

[2] Kempton, J. H., "Scientors appear in the Southwest." *Ibid.,* Vol. LXVI, No. 1711, pp. 354–355, Oct. 14, 1927.

ful youth, tread upon as many corns east as west of the Mississippi River. Be that as it may, if the wild scientists of the woolly west desire to pass resolutions to protect themselves from outside aggression and internecine strife, we should expect such ambition for self-improvement to be lauded rather than condemned by the cultured exponents of an older civilization east of the Alleghany Mountains.

In defense of the southwest code, we note first that it is specifically stated to be "tentative." We take this to mean that any of the rules may be altered or stricken out to which there is sufficient objection at home or abroad. Furthermore, it appears that the code is intended by members of the Southwestern Division to apply only to themselves; as we understand it, they have no intention—and probably no hope—of applying their reforms to scientists at large. There is, therefore, no occasion for immediate alarm, unless it be on the part of individual eastern scientists who intend, for climatic or other reasons, to migrate to the Southwest.

Now it would be invidious to intimate that Dr. Kempton would personally violate, or condone the violation of, any of the rules in question. We interpret his attitude rather as a kind of Menckenese objection to the appearance of anything savoring of a Rotarian philosophy among scientific men. For this point of view there is much to be said. But as between the two extremes of super-sanctity and sub-Menckenism we plead for a carefully weighted mean.

We should like to believe that scientific men as a class are above the need of a code of ethics. But the enthusiasm with which we commonly refer to an admired colleague as a gentleman *and* a scholar seems to involve a tacit admission that the virtues connoted by these two terms may exist separately as well as in combination.

It may be urged that, if a scholar be not already a gentleman, he can not be made one by any array of rules or resolutions. Alas, too true! But it would appear advantageous at any rate to have a definite code, by which one might decide for himself whether or not he is a gentleman, instead of depending on his colleagues to tell him, which sometimes causes lasting embarrassment on both sides. Then, too, even if it be antecedently improbable that anything can be done about the ethics of the present generation, there is the coming generation to consider—the nascent Ph.D.'s and innocents yet unborn. Is it not our duty to provide that they may learn by precept what it is not wholly certain we can teach them by example?

If, in view of these weighty arguments, it should seem desirable to follow the lead of the physicians, southwestern scientists and other groups of professional men in semi-public service, and to adopt a code of ethics to apply to scientists at large, we propose that, somewhere near the bottom of the list of needed reforms, the following be included:

Rule 160z. Scientific men shall be restrained from flailing each other through the medium of the press. The following penalties shall be provided:

(a) For gentle sarcasm the offender shall be given n black marks, in a large book to be kept by the Secretary of the National Research Council.

(b) For open satire he shall be given black marks to the number of $2n+1$.

(c) For burlesquing or lampooning colleagues, he shall receive a number of black marks to be represented by the expression

$$-\log\frac{1}{(n+1)^{10}}$$

(d) For comparing colleagues, by direct statement or implication, with realtors, insurance brokers, chirotonsors or morticians, he shall be turned over to the ministrations of practitioners of said professions, successively, in the order named.

In conclusion, I hasten to specify, on behalf of both Dr. Kempton and myself, that such rule shall not be retroactive in its application. *Ex hoc malo proveniat aliquod bonum.*

ROBERT C. MILLER

UNIVERSITY OF WASHINGTON,
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SCIENTIFIC BOOKS

Les Physiciens Hollandais et la Methode Experimental en France au XVIIIème Siécle. Par PIERRE BRUNET, Paris, 1926.

An Introduction to the Study of Experimental Medicine. By CLAUDE BERNARD, translated by HENRY COPLEY GREENE and LAURENCE J. HENDERSON. New York, Macmillan Co. 1927.

THERE never was a time when man did not use the experimental method of investigating his environment. There never has been a time when man did not form hypotheses on observations made thus and in other ways. An editor of Bacon's *Novum Organum* more than a hundred years ago remarked that Sir Isaac Newton had a very extraordinary method of making discoveries. When he was engaged in his famed inquiries about light he seemed first to have imagined in his mind how things were and afterwards contrived his experiments. Newton boasted he made no hypotheses, but no mind will work without hypotheses. There can be no doubt Newton made hypotheses both before and after he contrived his experiments on light or gravitation either. It is remarkable there was once a majority of scientific men who, less than a hundred years ago, talked about the experimental

method in such a manner the inference was justified that obtaining knowledge by the experimental method is the only way of obtaining knowledge. Claude Bernard was born in the year 1813; in that year Dr. Shaw wrote the above in the appendix he added to his translation of the *Novum Organum*. Magendie was Bernard's teacher and the inheritor of what the Dutch had been so instrumental in introducing to the French a hundred years before. Paul Bert, who succeeded to the chair of his master, says this book of Bernard's, published first in 1865, struck cultivated minds with admiration and astonishment, and it may be added threw many of the admirers off their base and into a foolish exaggeration which is not apt to be repeated now, sixty years later that it appears in the admirable form given to it in this translation.

It is seldom it is thought worth while to republish any work of medicine or general science after sixty years, but this work is in the same class with the *Novum Organum*. I fancy it is the lingering tradition of the mental obsession to which Bert refers we may look for the interest the publishers expect. It is opportune that we have Brunet's book to enlighten us as to how the experimental methods entered France so long before Bernard made it unreasonably dominant, an exaggeration against which he repeatedly protests. Such exaggerations are always to be expected to attend the advent of every manifest step forward in the progress of thought and the achievements of men, but we must realize that Bernard has no claim and made none to having first introduced modern experimental methods in France.

When the House of Hanover crossed the channel to govern England there was a large following from the Netherlands, though perhaps not so large as Holland had sent over with her Prince of Orange and long before that the English Puritans had fled to the Low Countries from the British Isles, so there had been large and frequent interchange between the two nations for many generations before. Even the war between the Dutch and the English on the water had done something. It was not broadsides alone of shot they had fired into one another. It was ideas too they had exchanged after Lord Bacon's death. The new learning and the models by which Bacon and Newton shaped it made a deep impression on the Dutch, but they had no thought of not making hypotheses. Much which Newton called mathematics, others called reasoning, they insisted. Although the mathematician, left to his own processes, "never deviates into sense," he must have a beginning that is to him rational if not sensible, while the result to the layman is neither; but sensible is a word that long since has begun to be slippery in usage. Physics formed the basis of Newton's mathematics and reckoning from it was more exactly done than by hypotheses, but in mathematics hypotheses also have to be used. The Dutch followers of Newton saw more clearly than some of his later English, but s'Gravesande declared that in order to arrive at the admirable wisdom of Newton every physicist should see to it that his reasoning was not founded on simple hypotheses alone. At Utrecht they instituted a large laboratory supplied with instruments of every kind then procurable for the study of physical phenomena, yet s'Gravesande laid down the maxim that the student should try to imagine in his own mind how the phenomena he was about to investigate could arise. Despite his warning that some method more reliable should form the basis of his projected work, six rules for the use of hypotheses were given by the Dutch savant and Brunet says he was apparently not far from thinking that "truths" themselves of physics are only high probabilities. Consequently the results of the experimental method are naught but hypotheses, however close to the truth they may come. We can not deny the experimental method is more often the origin of theories than of facts. Thus early in the Baconian and Newtonian creed did heresy arise when transported to the continent by their disciples. Nevertheless it was they who transmitted to France long before Claude Bernard a rational experimental method. It was not Bernard who was responsible for an irrational experimental method which caused much of the falling away of students in science in France in the latter part of the last century rather than the Franco-Prussian war.

Not only did primitive man experiment with the experimental method, but when science dawned in our civilization it was not entirely neglected by the old Nature Philosophers, though we have scant record of any. Hippocrates and Celsus, however, are recorded as having resorted to it in medicine and Galen was one of the greatest experimenters who ever lived. Neither Bacon nor Bernard were the earliest pioneers, nor the Italian School. Men believed Bernard had but followed in the path of Bacon, but he did far more than that. He was the most important of his critics. He was no bigot who believed that pure induction from observed facts can often lead directly to the truth. In this many, who have insistently quoted him for two generations of scientific men, have totally misapprehended him and English readers are put in the way by the present volume of avoiding this error. As to his inspiration by Bacon, nothing could be more erroneous. The chief criticism which Bernard aims against the methods of modern science is not against the neglect of experimental science which had

long flourished in France before him but specifically against the mistaken ideas of Bacon, evidently motivated by the abuse of the precepts of the inductive methods of research. In the introductory chapter of his work he insists that "the experimental method is nothing but reasoning by whose help we methodically submit our ideas to experience—the experience of facts. . . . The experimenter must be at once theorist and practitioner. . . . It is impossible to devise an experiment without a preconceived idea."

The chances were forty years ago that a student on entering a laboratory for the first time was adjured by the director to approach his problems with an open mind almost so insistently that the student had reason to believe he was to approach them with an empty mind. He must have no preconceived ideas as to the solution of them. This was mere drivel. It put a premium on infancy and idiocy. Claude Bernard was supposed to be the protagonist of such doctrine, for the history of medicine, especially of science, was not wide spread among laboratory workers in those days. Bernard insists "facts are necessary materials, but their working up by experimental reasoning, *i.e.*, by theory, is what establishes and really builds up science." Bernard seems, by the frequency with which he sounds this note, to be protesting against certain pernicious tendencies in French methods of the pursuit of science. It was the revival of a new science in the Netherlands, which was the origin of its juvenescence in France. There were more solid cultivators of it than Voltaire, but when he returned from England he made the ideas of science he had imbibed there popular. Probably his friend, Mme. du Chatelet, understood them better, but he was the greatest propagandist of them all, so that through him a very much more extended stratum of the people became acquainted with the ideas, which several years before had arrived by the way of Holland and were thus given a wider circulation than otherwise would have been the case from the introduction Nollet was able to give them among learned men.

Descartes and his philosophy had had a great effect on the teachings in the universities of Holland, but most of the physicists there appreciated the inefficiency of his doctrine. Boerhaave in his doctor's thesis in 1693 had already published matter in which he dwelt on the importance of the experimental method in medical research. In less than ten years he had acquired a great reputation at the University of Leyden, which extended throughout Europe. Leeuwenhoek had already communicated his observations with the microscope to the Royal Society of London, but it was in 1715 that s'Gravesande was appointed secretary to the embassy sent to congratulate George I on his accession to the crown of England. By means of one of the students he had had in the Low Countries he gained the acquaintance and the esteem of Newton and became a member of the Royal Society. When he returned to Holland Boerhaave had pronounced a discourse *De Comparando in Physicis,* which acted as a sort of primer of science for future work in Holland. After that the names we know of those who became indoctrinated with the ideas of the experimental method in science there are numerous. Thither Voltaire repaired to consult Boerhaave about not his health alone, but about the new things in science. In France already Nollet's influence had become marked among men of science. When the instruments then in use in the neighboring country became known in France they were constructed with more precision there, but Brunet does not forget the indebtedness not alone of France but of England and Holland also to the Italians, Galileo, Torricelli, Redi, Borelli. Milton drew on Italian culture for his great works in literature. Harvey, Willis, owed a deep debt to them. Later, however, the current of advance in methodology in science flowed through England, and with the impulse of Bacon's crude revolt from scholasticism behind them the new departure found its way to France very largely through Holland.

It was Boerhaave and s'Gravesande and Muschenbroek and doubtess many others who introduced and developed an admirable experimental method in the early part of the eighteenth century in Holland. They yield in no way to their great protagonists in England in that acuity of intellect, which manifests itself by talent and common sense rather than by genius. They escaped many of the errors of Bacon and Newton. They got their cue from them, but when the mantle fell on their shoulders science made vast strides in the Netherlands. In its earliest years in France there had already been some instruction given and some exposition of the experimental method by Polinière, but already the fame of Boerhaave was spreading there as well as elsewhere in the continent and in England and students were going to him from the latter country, whence at the beginning of the century he had drawn his first inspiration. Vaillant, who had been fired by the lectures of Boerhaave, left a posthumous book in the care of the latter, a confidence which Brunet thinks exceptional in a Frenchman for a Dutchman at that time, but its title has a significant interest for us. Boerhaave, five years after Vaillant's death in 1722, published it at Leyden and Amsterdam in 1727 as the *Botanicon Parisiense.* In France Castel had been provoked to attack Boerhaave for too much worship of Newton as the greatest of physicists and especially because he wished to banish every sort of hypothesis, which exclusion Castel was quite right in declaring often stops access to the truth, claiming that

even Boyle in England had given utterance to much of the same criticisms. But the work of Vaillant found its way into France and Noguez in 1725 translated a work of Niewentyt and in 1731 Boerhaave was elected to membership in the Academy of Sciences at Paris. Nollet, in 1738, was made the incumbent of a public chair of experimental physics in Paris founded by Cardinal Fleury. It was in Italy, in Florence, however, that there existed the oldest Academy of Experimental Physics in Europe.

JONATHAN WRIGHT

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SPECIAL ARTICLES

LONG-LIVED CELLS OF THE REDWOOD

THE principal features of long-lived cells in the massive stems of cacti have recently been described by the senior author.[1] Medullary cells of the tree cactus (*Carnegiea*) were seen to remain alive for periods well over a century and the results of the examination of elements of all ages indicate active enlargement during the second half of the century.

Another cactus, the melon cactus or bisnaga (*Ferocactus*), which has an ovoid-cylindrical trunk was found to include similar medullary cells of great age which, however, ceased to grow after the first decade. The development of the outer cortex as a layer several inches in thickness is of such character as to demonstrate that the cortical cells have a similar period of enlargement and survival.

In all of these cells the carbohydrate components, pentosans and hexoses progressively decrease with age. The fatty substances, or lipins and nitrogenous substances, change least.

Transformation of sugars to wall-material with consequent thickening is apparent in *Carnegiea* and in the medulla of *Ferocactus*; crystals of salts accumulate in all cases. It was notable, however, that in the cortex of *Ferocactus* the disappearance of the carbohydrates extends even to the walls, which are thinner at a hundred yards than at ten, suggesting the liquefaction and removal of pentosans. It seems probable that the only consumption or use of the lipins from these cells is of that which may be in the walls and cytoplasm, and that these substances in the nuclei are but little affected.

That ray cells may attain an age of many years in the whitish sap wood of tree trunks is implied in the writings of many authors. We can not find any definite statement of living cells in heart wood, although Strasburger's account of starch in trunks of the red beech 125 layers from the surface suggests that these cells might be alive.[2] The negative assertion that "only the outer layer of the wood composed of the more recently-formed annual rings thus contain living cells and constitutes the splint-wood" made in the 14th German edition of Strasburger's text-book of botany and found on p. 158 of the 5th English revision as translated by Lang in 1921 may be taken as a correct presentation of present knowledge of this matter.

Certain features of behavior of the trunks of the California Redwood uncovered by our study of the hydrostatics of this tree led us to examine the parenchymatous cells of the trunks. There are two different types of living cells in newly-formed secondary xylem of *Sequoia sempervirens:* wood-parenchyma cells that stand in vertical files scattered among the tracheids, and ray-parenchyma cells. As in most other woody stems, the wood-parenchyma and ray-parenchyma cells of the alburnum or whitish sapwood are living and densely packed with starch.

The change from alburnum to duramen (heartwood), macroscopically recognizable in *Sequoia* by a brownish-red coloration of the duramen, is accompanied by a disappearance of starch and protoplasts from all wood-parenchyma cells and the formation of an orange-colored resin that completely or partially fills the lumen of the wood-parenchyma cells. For this reason wood-parenchyma cells are often called resin cells. The transition from alburnum to duramen is also accompanied by a disappearance of starch from the ray-parenchyma cells but this disappearance of the starch is not always followed by death and disintegration of the protoplasts. These living ray cells of the duramen have a thin layer of cytoplasm next the wall, a conspicuous nucleus, and a large central vacuole. The ray cells may remain unchanged for a long time and we have observed ray-parenchyma 70 layers deep in the heartwood with clearly defined protoplasts and apparently normal nuclei. As the sapwood in such trees included 21–23 layers these cells were about a century old. Cells of older annual increments in these stems also appeared to be living but this could not be determined with certainty since the granular nature of the cell contents obscured the nuclei. Some trees have ray-parenchyma cells that show a granular cytoplasm and small droplets of resin shortly after the transition from duramen to

[1] D. T. MacDougal. "Growth and Permeability of Century-old Cells," *Amer. Naturalist* 60, 393–415, 1926; and Frances L. Long, "Characters of Cells attaining Great Age," *Amer. Naturalist,* September–October, 61, 385–406, 1927.

[2] Strasburger, E. "Ueber den Bau und Verrichtungen d. Leitungbahnen in den Pflanzen." Pp. 274-275. 1891.

alburnum; still others have the same behavior in ray-parenchyma and wood-parenchyma and both elements in the outermost ring of heartwood contain resin only.

The reasons for this marked variation in the behavior of ray-parenchyma cells are as yet unknown. The age of the tree does not appear to be a factor, for we have found both conditions in young and in old trees. Neither is there any sharp correlation with the environment. Trees high on the dry flanks of hills or deep in the canyons of this vicinity may have living ray cells deep in the duramen.

That these cells should survive during the pronounced changes in chemical constitution of the wood, altered composition of the sap and lessened oxygen supply is a remarkable occurrence. The nuclei undergo but little change and a superficial examination suggests consumption of the carbohydrates. The well-known abrupt diminution of starch in the rays occurs in the redwood at the stage of transition from sap to heartwood. The ratio of length of life of these cells to that of their growing period is the highest known. Full size is reached almost at once—within a few days—life may continue for a century or four thousand times the duration of the growing period. In *Carnegiea* medullary cells continue to enlarge for a century; in *Ferocactus* life continues for a period ten or twelve times the growing period which extends over a decade.

It is notable that the known long-lived cells of plants are all of the simple parenchyma type as in contrast with the highly specialized long-lived cells of the brain and heart of vertebrates.

The medullary cells of *Carnegiea* which grow for a century, as might be expected, retain their embryonic character. Those of the medulla and cortex of *Ferocactus* do not. The long-lived cells of the redwood lose their capacity for division at an early stage and play no direct part in the formation of calluses, or other regenerative action so marked in the redwood. In what way the existence of these numerous strips of living cells in the heartwood, which may reach an age of more than a century, affect the pressures and movements of liquids and gases in this tree is yet to be determined. The facts presented seem to constitute the first announcement of living cells in heartwood, as well as an extension of knowledge of the occurrence and behavior of long-lived cells.

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GLUCOSE AS AN ANTAGONIST

FOR some years the senior author has been engaged in the study of the habits and means of control of the onion root maggot (*Hylemyia antiqua* Meig.). Experiments have included extensive investigation of the chemotropic responses of the flies to various substances. Cane molasses has always proved a very satisfactory attractant and NaCN has long been known and used as an effective insect poison. In consequence, a mixture composed of ¼ ounce of NaCN, 1 pint of molasses, and 1 gallon of water, was included among the various poisoned baits it was desired to test.

In the summer of 1925, Mr. K. Stewart, working on the same problem, found that the NaCN-molasses bait gave very satisfactory results, ranking first of all the materials tried in the average daily catch of Diptera. These records were obtained by the use of wire fly-traps of the Minnesota type placed in the onion fields on the College farm, with the various poison baits in glass dishes under the traps.

The NaCN-molasses combination, as one of the most promising of the mixtures under trial, was selected by the senior author for further experimentation in 1926. It continued to give very satisfactory results so far as the catch of flies was concerned, but some doubt was experienced as to the extent to which this mixture was acting as a killing agent. Accordingly, cage experiments were started indoors in order to settle this point. It was found that while the NaCN combination was even *more* attractive to the flies than molasses and water alone, its toxicity was practically nil. Upon uncorking a bottle of the mixture which had been well shaken and then allowed to stand for several days, a distinct odor of ammonia was noticed, while that of HCN could no longer be detected. Moistened litmus paper gave the alkaline color reaction when held near the mouth of the bottle. Apparently, the –CN group had been decomposed, giving rise to ammonia, the evolution of which gas no doubt explains the enhanced attractiveness to the flies of the poisoned bait over that of a simple molasses-water solution.

The most probable explanation of the production of ammonia in the above mixture appears to be that of interaction between the NaCN and the glucose of the molasses to yield glucose cyanhydrin, which subsequently hydrolyzes, as follows:

$$NaCN + H_2O \leftrightarrows NaOH + HCN$$

$$CH_2OH(CHOH)_4 \cdot CHO + HCN \leftrightarrows CH_2OH(CHOH)_4 \cdot CH(OH)CN$$

$$CH_2OH(CHOH)_4 \cdot CH(OH)CN + H_2O \leftrightarrows CH_2OH(CHOH)_5 \cdot COOH + NH_3$$

Glucose solutions to which NaCN was added were found to give essentially the same results as did the molasses bait, but to a more pronounced degree. Further confirmation of the hypothesis respecting the origin of the ammonia is afforded by the results obtained with a mixture of cane-sugar solution (of the same specific gravity as that of the molasses used) and NaCN in the proportions of ¼ ounce of the latter to 1⅛ gallons of the former. When this mixture was used in cage experiments it was found that the HCN given off usually was sufficient to kill most of the flies without their having the opportunity to start feeding. Sucrose, having no free carbonyl group, would of course not undergo the cyanhydrin reaction.

In connection with the above, it is interesting to recall that Rasputin, the Russian monk who dominated the court of the late Czar, is reputed to have used glucose as a protection against sundry attempts upon his life by means of poison. Such results as have been described would suggest that this precautionary measure was well taken, in the case of NaCN at least. In addition, the findings of Heinekamp[1] respecting the resistance of various types of fowl to strychnine, furnish evidence that here also glucose, or its polymer glycogen, may have a rôle as antagonist in the case of the alkaloids as well as in that of the less complex poisons.

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GEOCHRONOLOGY AS BASED ON SOLAR RADIATION

In this journal of 1920 was mentioned a part of my plan for an investigation of certain laminated clays in North America.

During a previous visit in this country in 1891, I had, at several places, observed laminated clays similar to analogous late glacial melting sediments in Sweden, which I had found, after long-continued investigations, to represent the annual deposition from the melting water along the border of the retreating ice-edge.

By help of a certain graphic method for the comparison of the sharply marked annual layers or *varves,* I had succeeded in identifying such varves from one point to another and ultimately worked out a systematic plan for the elaboration of a continuous time scale. This was in the main carried out in 1905–06 on the basis of field measurements made, for a considerable part, by a great number of able young assistants. During the following years this standard line was completed at many places. I thus succeeded, step by step, in tracing the recession of the ice border and the immediately following transgression of the clay-varves over one region after the other, until the whole line from southernmost Sweden to its central parts had been controlled.

As was to be expected, the lowest and oldest clay-varves were found to be deposited in the southernmost part of the land, while the great ice-sheet still covered the rest. By following the clay transgression step by step, the whole staircase-like series was gradually built up, forming a continuous and exact time scale for the last ice-recession across Sweden up to a certain year when a great ice-dammed lake in central north Sweden was drained off, and a thick annual varve was deposited, which has been chosen as representing the very end of the late Glacial Epoch proper.

For many years I tried in vain to find out any means of determining the length of this Postglacial Epoch, until one of my most successful assistants, R. Lidén, discovered, in the northern parts of Sweden, measurable annual varves which represent that epoch.

Of varves from this epoch, I measured in one section at the Indal River somewhat over 3,000, but it was along the Angerman River that Lidén quite independently succeeded in finding and working out practically the whole of the Postglacial varve succession by a careful and painstaking combination of a long series of sections, showing the total length of the Postglacial Epoch to be about 8,700 years.

What made his work especially important is that there seems to be no other place in the world, not even in Sweden, where there is any possibility of finding such a continuous varves series for the whole of the Postglacial Epoch.

By noting the position of the bottom varves, the ice-recession in Sweden has been determined in detail at certainly more than 1,500 different localities, while sections down to the very bottom varve elsewhere—excepting in Finland—have but rarely determined the front margin of the lowest varve.

Thus, along the Connecticut Valley, according to Antevs' exceedingly interesting and comprehensive varve measurements, the bottom of the clay layers was so seldom exposed that the bottom varve, which marks the stage of the ice-recession, could only be determined at one point along the whole of the southern four fifths of the measured line of ice-recession. Even the remaining fifth part of the line, which is north of a gap in the section not represented by clay deposits, the bottom varve was located at only five points.

[1] Heinekamp, W. J. R., "The Resistance of Fowls to Strychnine." *Jour. Lab. and Clin. Med.* 11: 209–214 (1925).

ough in this region it is not yet possible to know ly the rate of ice-recession, the numerous mea- ents of long varve series have afforded valuable rial for detailed teleconnections with the time in Sweden and a thorough and very satisfactory cation with respect to the thickness variation of arves of some otherwise uncontrolled parts of Swedish time scale.

rthermore, though it has not been possible, even ch an admirable varve tracer as Dr. Antevs, to a continuous varve series through the late glacial of the time scale in Canada, this is, happily h, of less importance since the extended telecon- ons with the already minutely controlled Swedish scale afford an equally good but much more con- nt and time-saving method.

us it is neither necessary nor advisable to use tain estimates or perhaps quite erroneous as- tions for filling out gaps between actually mea- varve series, if the whole sum really is to be worthy.

rthermore, considering that the initiative, the nization and method of the investigation are ly of Swedish origin, and, as it seems, it is only weden that the time scale can be continued all the up to our own age, it may be fully justified to this chronologic standard line the *Swedish Time* , even where it is identified and completed by llel measurements in other countries.

e main purpose of an exact international chro- gy must be the reference of different kinds of ts everywhere to one and the same time standard. e necessary condition for the introduction of such me scale was, of course, the possibility of identifi- on, even at great distances, of synchronous varve tions. This condition seems now to be realized. hus, almost all of the varve series measured in by the little Swedish expedition to North rica have been with certainty identified with sponding series of the time scale in Sweden, as trated by some examples representing a few sands of years, published in the *Geografiska aler,* Stockholm, 1926.[1] Quite lately the author also succeeded in identifying a series of nearly varves, carefully measured in Argentina by one y former pupils, Dr. Carl Caldenius.[2] Also a ription of E. Norin's varve measurements in northwestern Himalaya, carried out and identified by him in 1925 and 1926, will soon appear in the same journal.

Considering that the distances between Sweden and the other regions are no less than 6,000–14,000 km., the similarity of more than 80 per cent. of the whole varve number between the identified varve series is very astonishing but quite convincing with respect to the long and uniform succession of characteristic varve variations, which, naturally enough, have not been possible to connect in more than one single way. It is especially noteworthy that this similarity continues century after century, thus, as it seems, putting it beyond every doubt that only a real identity is able to explain such a considerable sum of similarities.

Here it must also be emphasized that it was first after many totally vain attempts that the striking correspondence was discovered, and then was at once found to be continuous over practically the whole of the varve series.

A close study already made of the published diagrams will soon show that a mere accident here must be totally shut out. This remarkable coincidence of such rapid variations at such considerable distances, caused by simultaneous ice-melting, seems not to be explicable in any other way than by variations in the amount of heat from the sun. Thus in reality, we have to do with nothing less than a gigantic, self-registering thermograph, showing the variations in the radiation from the sun.

With respect to the physics of the sun, it is of interest that there exists an observed biennial variation as well as the annual one, this latter being indicated by the connection of the varve variation of the two hemispheres, so that the north and south summers, with their closely corresponding varves, point to a natural *thermal year.* Still it is undetermined whether that year begins with the one hemisphere or the other.

It may be of interest for the elucidation of this question to include also the southern part of the Cordilleran observations which are analogous to the renowned measurement of the solar radiation which, for two decades, have been executed on the sunny heights in the far West of the United States.

Whatever may be the astronomic cause of the annual solar variation, it seems likely that its beginning and end probably are to be found somewhere between the north and south summer, or not far from the equinoxes. When this has been fully stated, our annual means of temperature ought to be calculated for the natural thermal year, which ought to give much more characteristic results than the means of the arbitrary fiscal year.

G. De Geer, "On the Solar Curve, as Dating the Ice the New York Moraine and Niagara Falls through wedish Time Scale." Data, 9. *Geogr. Ann.,* Stock- Vol. 8 (1926), pp. 253–284, Pls. 1–3 (varve dia- s and map).

G. De Geer, "Late Glacial Clay Varves in Argentina, ured by Dr. Carl Caldenius, dated by Means of the Curve through the Swedish Time Scale." Data, 1. *em,* Vol. 9 (1927), pp. 1–8, Pl. 1 (varve diagrams).

Though it may be granted that the varve variation as to its dominating features all over the earth is a function of the sun radiation, at the same time it is still more or less influenced by local factors, such as the bottom topography at different localities together with changes in the direction of the melting rivers, the varying composition of the morainic material from which the clay is washed out, and other circumstances. By parallel measurements at different localities, many such deviations from the true solar curve will probably be eliminated, but meanwhile it is not advisable to transcribe the varve thicknesses in figures intended for smoothing calculations.

Yet a really convincing graphic connection already observed makes it possible for the first time to introduce in an exact way the time factor into a great number of geophysical investigations hitherto beyond our reach. Thus, to mention a few examples, the possibility of mapping and dating synchronous land ice borders over great areas in different regions of the earth enables us to take up, with respect to the physics of large ice bodies, a rational study of ice movement, of ice extension, and of its recession as a function of melting as well as of fracturing.

In the papers quoted, some hints are given concerning the use of time determinations for a closer geophysical study of other processes, of inorganic as well as of organic nature, such as the evolution of our actual climate and the erosion along our rivers as well as our lake and sea shores. In these papers was especially mentioned the magnificent example of Niagara Canyon, the age of which has now been geochronologically dated by means of good varve connections. Thanks to the excellent measurements by American and Canadian geologists, we have here a prime example of the amount of river erosion under certain conditions during a non-determined space of time.

Here may be mentioned furthermore the new possibilities of studying, step by step, how the ice recession was followed by the formation of soil and vegetable mould and the immigration of the whole flora and fauna within the former great ice deserts, which afford unique possibilities of supplying certain branches of geology with exact geophysic studies.

GERARD DE GEER

THE NATIONAL ACADEMY OF SCIENCES

(*Continued from page 434*)

The meliolineae: FRANK LINCOLN STEVENS. The Meliolineae are highly specialized biologically. This results in an isolation comparable in general to local geographic isolation. A species on a given host remain limited to that host over long periods of During the lapse of time modification of the fungu curs resulting in distinct varieties. Further changes to specific, even generic, differentiation. It thus hap that distinct but related forms are found on one species. The evolution of these occurred during the ancy of this phylum on this host. The occurrence complexes of numerous related species on a given family is very striking. No significant deductions geographic distribution are evident; indeed, the striking fact is the relative evenness in which the ous genera and species are distributed over the Me world. Apparently evolutionary tendencies have ated in diverse areas in much or quite the same The course of evolution appears to have been from non-hyphopodiate, 8-spored forms with persistent and spores of variable septation to hyphopodiate, spored forms with evanescent asci and spores of de septation, leading to the establishment of six ge within the group. It appears that these genera ar polyphyletic origin, each having arisen several, pe many times, from different ancestral stocks.

Some studies on the composition of vegetative fruiting plants: E. J. KRAUS (introduced by John Coulter). Accumulating evidence on many type plants shows that there is often a marked tendency the gradual accumulation in the proportion of carb drates and carbohydrate-like compounds in excess of nitrogenous constituents as the fruiting stage approa In some instances the differences in composition more specifically related to the relative solubility of nitrogenous and carbohydrate constituents than changes in actual proportions of these substances, more fruitful plants containing a larger proportion relatively insoluble substances. The general trend of results is the same whatever the external environme factors concerned in prolonging or shortening the v tative period.

Progress with work on disease-resistant plants: L JONES.

The dynamics of plant growth to soil conditions, special reference to oxygen and carbon dioxide: Bur E. LIVINGSTON (introduced by W. A. Noyes).

The quantum efficiency of the photochemical deco sition of anhydrous formic acid: WESLEY NORMAN and W. ALBERT NOYES, JR. (introduced by J Stieglitz). The number of quanta required to de pose one molecule of anhydrous formic acid in the l state has been determined. The light intensities measured by means of a thermocouple and the transm light subtracted from the incident light to obtain amount of light absorbed. The source of light used a quartz mercury arc lamp. The amount of decom tion is determined by gently boiling the liquid until decomposition products are removed and then by me

the pressure increase in a constant volume. As ed out by previous workers, the hydrogen formed g the reaction does not appear as a gas but prob- reduces formic acid. As the average of several de- inations it is found that 2.21 quanta are required e average to decompose one molecule of formic acid. ssible effect of dust is indicated as the above figure ne 1.57 when dust was present. When only wave- hs longer than 300 m. are used the number of ta per molecule is 3.25. Similar measurements were on formic acid vapor. To measure the decomposi- products, the vapor was condensed with liquid air the gaseous products swept into a McLeod gauge a Toeppler pump. The process was repeated until further pressure increase was noted. Addition of fully purified hydrogen showed that hydrogen was up in this case also. The number of quanta re- d to make one molecule decompose in this case was Measurements with the solid were inaccurate due fficulty in determining the amount of reflected light. found that there is no obvious relationship between energy required to decompose one molecule of liquid, energy required to decompose one molecule of vapor the heat of vaporization. Possible effects of the in the two cases will be discussed.

lectrochemical theory of oxidation and neutralization ions: W. H. RODEBUSH. The electromotive series of metals can be extended to include most of the ele- s and groups of inorganic and organic chemistry. groups at one end of this series will be recognized oth strongly oxidizing and strongly acid forming. is not a coincidence. These groups have a deficiency egative electricity. This deficiency may be satisfied hree ways: (a) A hydrogen ion may be lost; (b) an tron may be acquired; (c) a neutral atom itself de- nt in electrons may be given up. (a) is a typical tralization reaction and (b) and (c) are characteris- oxidation reactions. Numerous examples can be d.

rogress in the concentration of illinium: B. S. HOP- s. Illinium was detected in portions of rare earth terial that had been derived from monazite sand, the ration being accomplished by fractional crystalliza- mainly as double magnesium nitrates and as the nates. The concentration obtained by these means, le adequate for identification, probably did not ex- one per cent. of illinium. Efforts are now being ted to increase the amount of illinium in various s. (1) Fractionation as double magnesium nitrate bromate is being applied to a large quantity of azite residues. (2) Other methods of fractional tallization are being used, such as the perchlorates, cyanides and dimethyl phosphates. (3) Determina- of the relative basicity of illinium as by the frac- l precipitation with NH_4OH and $NaNO_2$. (4) ration of illinium from its neighbors by means of varying ionic velocities. (5) Other minerals, espe- y those rich in neodymium and samarium, are being examined for illinium. The results so far obtained indicate that the best method of concentration is by means of the fractionation as double magnesium nitrate and as bromate, although the fractionation as dimethyl phosphate looks promising. The ionic migration method has possibilities which are attractive, although the results so far obtained are not conclusive. It seems probable that the basicity of illinium will place it between neodymium and samarium, although work on this phase of the problem is especially tedious. One quantity of American samarskite was tested for illinium with negative results, but the present indications are that fergusonite contains a larger proportion of illinium than monazite. Other ores such as cerite, allanite, gadolinite and tscheffkinite are now under investigation as possible sources of illinium.

The critical stage of the earth's megadiastrophism: T. C. CHAMBERLIN.

Factors governing the low temperature carbonization of high oxygen coals: S. W. PARR (introduced by W. A. Noyes).

Hydrazoic acid; an ammono nitric acid, a nitrous acid hydrazide and an ammono hyponitrous acid: E. C. FRANKLIN.

Isolation of the alpha form of methyl alpha glucoheptoglucoside and its acetate: C. S. HUDSON.

Glacier motion as a type of rock deformation: ROLLIN T. CHAMBERLIN. In an endeavor to decide between viscous flow and crystalline yielding, measurements of the internal shearing in various glaciers were made with a self-recording clockwork apparatus. The results show that slipping takes place along definite shearing planes, at times gradually and at other times by distinct jumps. The capacity to withstand a certain amount of growing stress before yielding is indicated. Four manifestations of glacier movement are recognized: (1) Solid flow by idiomolecular exchange between ice crystals, (2) solid shearing of aggregates of granules, (3) intermittent slip along well-developed thrust fault planes, and (4) sliding of the whole body of ice over the rock beneath. Here is a rock of simplest sort actually undergoing deformation before our eyes. It gives concrete illustration of many of the phases of earth deformation which have been interpreted chiefly from results remaining from the past.

Concerning the metal in meteorites: GEO. P. MERRILL (with lantern slides). The paper gives a brief résumé of opinions relative to the metallic constituent of stony meteorites, and dwells mainly upon its physical properties as compared with artificial material. It is shown to partake of the nature of artificial so-called wrought iron and undergoing decided changes on fusion under ordinary conditions. Particular attention is, however, called to the position of the metal relative to the silicate constituents and the conclusions to be drawn therefrom.

The influence of oscillating sea-level on the development of the continental shelf: FRANCIS P. SHEPARD (introduced by David White).

Lake Illinois and the problem of its duration: M. M. LEIGHTON (introduced by T. C. Chamberlin).

Evolution of the odd-numbered elements: W. V. HOWARD (introduced by H. S. Washington). When the isotopes of the elements which have been determined by Aston and others are plotted on coordinate paper with the atomic numbers as abscissae and the mass numbers as ordinates they are found to have certain definite relationships to one another. These may be expressed as follows: (1) The isotopes of the even-numbered elements between carbon and polonium form two groups of series, of which one group conforms to the equation $M = 2N + 4X$ and the other to the equation $M = 2N + 4X + 2$, where M is the mass number, N the atomic number and X a whole number between 0 and 11. (2) Each series corresponding to any given value of X in one or other of the two groups is terminated by an element which has an isotope with an odd mass number, and in some cases two such isotopes. (3) The odd isotopes of the even-numbered elements may lie immediately above the lowest and second lowest even-numbered isotopes or above either of these, but never occupy a higher place in the list of isotopes of any element. (4) The elements whose lower isotopes terminate one or more series have higher isotopes which begin one or two others. (5) No element has more than five even isotopes (xenon excepted). (6) The above rules do not hold in their entirety for the two series in which $X = 0$. (7) No odd-numbered element has more than two isotopes. (8) No odd-numbered element has an isotope with an even mass number. (9) No odd-numbered element has an isotope whose mass number is the same as that of any isotope of any other element, odd or even. (10) If an odd-numbered element has two isotopes, the following even-numbered element can not have more than one odd isotope. (11) The isotopes of all odd-numbered elements have a mass number which is less by one than one or other or both of the two lowest isotopes of the even-numbered element immediately following. These rules do not hold for nitrogen, the elements below carbon in the periodic table or the radioactive elements. By means of these rules the isotopes of those elements which have not yet been successfully attacked may be predicted with results which agree very closely with those of Russell. These relationships together with certain experimental results and the occurrence of the different elements in the earth's crust suggest that the odd-numbered elements were formed from the lowest isotopes of the even-numbered elements by the loss of a proton and an electron which combined to form atomic hydrogen. If it be assumed that this change has taken place, it is possible to account for the amount of water in the ocean and the great quantity of juvenile water which is being constantly added to the earth's crust. The process of magmation becomes one whereby disintegration of the odd-numbered elements at a comparatively shallow de below the earth's crust raises the temperature of rocks and at the same time provides water, which res in the lowering of the melting-point of those rocks that a magma may be formed. The theory of the ra active control of mountain building becomes one mountain building caused by non-radioactive disinteg tion of the elements. All the heat involved in mount building, the formation and rise of magmas, and the h radiated from earth's crust may be supplied from one cause alone.

Evolution of the odd-numbered elements: W. HOWARD (introduced by H. S. Washington).

The ordovician section of northwestern Illinois: E. ULRICH.

Some factors in rock metamorphism: DAVID WHIT

The Shinarump conglomerate and its associated ve brate fauna: E. C. CASE (introduced by F. G. Novy)

Influence of oscillating sea-level on the development the continental shelf: F. P. SHEPARD. Evidences changing relations of land and sea are found on alm every coast. Little has been done to determine whet such changes are dominantly a result of crustal warp or of the shifting of the sea-level. The problem has b discussed chiefly in connection with coral reefs. Si these reefs occur mostly in very unstable regions, results can not be said to be very satisfactory. There a terrace-like submarine platform around the vari continents, which is covered by rarely exceeding wa 600 feet. It is terminated by a fairly steep slope wh leads down to the deep ocean basins. This is known the continental shelf. A study of this feature sho throw light on the question of sea-level changes. Pr ously only small parts of the shelf have been conside with any care. The present study took in all parts of shelf. The results of the study lend much support to idea of shifting sea-levels. Hills, valleys, delta flats, races, are found in abundance along all parts of continental shelf. In connection with coral reefs stud it has been proposed previously that the sea was lowe by glaciation about 200 or 300 feet. Such lowering d not appear to be adequate to explain the features m tioned above, which are indicative of sub-aerial or littoral erosion and deposition. It seems probable t during much of the Tertiary period the sea-level w from 400 to 800 feet lower than at present. In test the validity of this hypothesis all varieties of coasts w considered, such as arid, humid, mountainous, low and forth. The characteristics of the shelf adjacent to e type of coast were compared with predictions based the various hypotheses for the origin of the shelf. each case the actual conditions appeared to fit the ne suggested hypothesis much better than the others. such considerable changes of sea-level have occurr many coastal features formerly ascribed to diastrophi can be explained without it.